THE ANALYSIS OF KNOWLEDGE IN JOHN STUART MILL AND WILLIAM WHEWELL

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THE ANALYSIS OF KNOWLEDGE IN
JOHN STUART MILL AND WILLIAM WHEWELL

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THESIS

The Analysis of Knowledge in
John Stuart Mill and William Whewell

(Abstract)

The phrase, "the analysis of knowledge," as it is used in the title of the thesis, refers to the conclusions arrived at by Mill and Whewell concerning knowledge. It also refers to the methods employed by the two philosophers in arriving at those conclusions.

For the most part, Mill and Whewell regarded themselves as philosophical adversaries. They were particularly conscious of being in disagreement about the origin and structure of knowledge, and the nature, grounds, limitations, and results of scientific and philosophical inquiry. But it is contended in this thesis that the views of Mill and Whewell on these matters are more in agreement than each supposed.

The major claim which I attempt to establish is that Mill and Whewell agree on eleven basic epistemological issues. In order to establish this claim I discuss their epistemological writings under three main headings--"The General Character of Knowledge," "The Data of Knowledge," and "The Methods of Knowing." I first present Whewell's position, and then show that Mill explicitly agrees with him on some points and that on others he cannot very well disagree. In order to show their agreement it is sometimes only necessary to set forth parallel expositions under the three headings. But sometimes it is necessary to resort to other devices. Three devices of which I make considerable use I call "decisions," "theory of knowledge," and "metatheory of knowledge." I wish now to state briefly what I mean by these expressions.

By "decisions" I mean statements upon which we are willing to act as if they were true, but whose truth we have not established. Whewell refers to such statements as "axioms," and claims that their truth can be established intuitively. But I prefer to call them decisions, because I am of the opinion that the most we can say about their truth is that if we accept them as true, then certain conclusions based on them can also be said to be true. We cannot demonstrate the conclusions unless we decide to accept as true the primitive statements upon which the conclusions depend.

The decisions which I discuss may be divided into two groups. One group makes up what I call "theory of knowledge." The other group makes up what I call "metatheory of knowledge."

By a "theory of knowledge" I mean a set of primitive statements which are logically prior to all the recognized sciences. These statements might be said collectively to constitute a definition of knowledge. By a "metatheory of knowledge" I mean the decisions which support a theory of knowledge. A major part of the defence of my claim that Mill and Whewell agree on eleven basic epistemological issues consists in showing similarities in their respective theories and metatheories of knowledge.

The similarity in their respective theories of knowledge consists in their subscription to the following views. First, knowledge consists in the relating of particulars, i.e., entities which can be perceived or thought as discrete. Secondly, the particulars related must be capable of appearing within conscious awareness. Thirdly, the particulars are related by means of activities of a knowing subject.

In their metatheories, both philosophers contend that the above views are supported by the following three considerations. First, there are particulars. Secondly, we perceive subjects and objects in which particulars appear as related. Therefore, for epistemological purposes at least, there are subjects and objects. Thirdly, there are valid methods of relating particulars. These methods are induction, deduction, and intuition. But in the present context it seems proper to stress only intuition, since it is the ground of the possibility of employing induction and deduction. I conclude, therefore, that the foundation of the epistemologies of Mill and Whewell is the decision to act as if the statement, "Whatever is intuited as valid is valid," were true.

In the central chapters of the thesis I do two things. First, I provide an exposition of Mill's and Whewell's views concerning the general character of knowledge, the data of knowledge, and the methods of knowing. Secondly, I try to work out something which they do not explicitly provide, namely,

their respective theories and metatheories of knowledge. As a result of setting forth and examining their analyses of knowledge, I conclude that Mill and Whewell agree on eleven basic issues.

These issues may be stated in the following manner. 1. The philosopher's task is to make explicit the grounds of the possibility of knowledge. 2. The term "knowledge" denotes what is provided by the recognized sciences. If we try to formulate a generic definition of the term which will hold for all of the recognized sciences, it becomes clear that we must offer two mutually dependent contentions. First, "knowledge" means "true statements known to be true of experience, actual and possible." Secondly, "knowledge" means "the relating of particulars." 3. The ultimate ground of knowledge is intuition. 4. Realism is a necessary assumption, at least where epistemology is concerned. 5. A correspondence theory of truth is the theory to be preferred. 6. The "spectator model" is the most defensible representation of the knowing situation. 7. There are three basic methods of knowing, viz., induction, deduction, and intuition. 8. The kinds of knowledge are related, and it is possible to construct a classification of them which shows how they are related. 9. The formal qualities of experience and knowledge are what the mind knows truly. 10. In order to provide a satisfactory analysis of knowledge it is necessary to make explicit the role of the knower, since the relating of particulars, which constitutes knowledge, is accomplished by means of acts of the knowing subject. 11. Epistemological systems are never complete. We cannot quite "close off" such a system. The major reason why this is so is that there are always statements which we know to be true, but which we cannot demonstrate to be true. These statements can only be incorporated within knowledge by an appeal to an intuitive knowledge of those truths which are basic to all others.

If it be granted that Mill and Whewell do agree on these items, then I make the further claim that to call Whewell a "Platonist" or "Neokantian" and Mill a "Positivist," as is sometimes done, is to make a distinction which is not very significant.

GRADUATE STUDIES

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Minor Subjects:

Systematic Philosophy and Metaphysics	--M.A. Degree, (University of Western Ontario)
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CHAPTER I

INTRODUCTION

In this thesis I propose to consider the analysis of knowledge in the philosophies of John Stuart Mill and William Whewell. As a preliminary to my main task, I shall undertake in the introductory chapter to do two things. In Part A of the chapter I shall state, in very general terms, the subject matter of the thesis. In Part B I shall set forth certain techniques which I have employed to facilitate exposition and, especially, to facilitate comparison and evaluation.

A. The Subject Matter of this Thesis

1. The Meaning of Epistemology in the Thesis

Ledger Wood defines "epistemology" as "the branch of philosophy which investigates the origin, structure, methods, and validity of knowledge."[1] If this be taken as a standard definition of "epistemology," then such a definition does not satisfactorily state what I mean by the term. "Epistemology," as I interpret it, denotes the terms listed by Wood, and also the nature, grounds, limitations, and results of knowledge-seeking activities. And while there are many kinds of knowledge-seeking activities, I am interested primarily in only

two such activities, viz., scientific inquiry, and philosophical inquiry.

2. The Meaning of "The Analysis of Knowledge" in the Title of the Thesis

The "analysis of knowledge," in the title of my thesis, denotes the conclusions reached by Mill and Whewell concerning science, scientific inquiry, and philosophical inquiry. And it also denotes the methods employed by them in arriving at these conclusions.

It will be noted, in Ledger Wood's definition of "epistemology," that he refers to it as investigation. Wood's definition recognizes something which writers in epistemology sometimes overlook, namely that the portion of any philosophy which we call its epistemological element is more than certain conclusions about knowledge. Epistemology, in addition to being a body of conclusions about knowledge, is also an investigation which leads to these conclusions. The "analysis of science," then, designates (1) the conclusions about science to which Mill and Whewell respectively subscribe. These conclusions are of two kinds, descriptive and prescriptive. (2) The "analysis of science" also designates the investigation which leads to the conclusions noted in (1). This is essentially only a difference of emphasis. In (1) the emphasis is on the analysis of <u>science</u>. In (2) it is on the <u>analysis</u> of science.

In addition to the above, I am interested in the conclusions Mill and Whewell reach concerning the nature of

scientific inquiry. And I am also interested in the arguments employed by them in arriving at these conclusions. An example will help to clarify the distinction. Mill holds that the scientist employs his Four Methods. These are methods of discovery as far as the scientist is concerned. But in discovering that these are methods of scientific discovery, Mill does not employ these methods. Mill's Logic does not result from the employment of the Four Methods, but from the employment of some other method. The analysis of scientific inquiry can, therefore, concern itself with the conclusions about scientific inquiry, and the process which leads to these conclusions.

It is my contention, furthermore, that we may discern two kinds of statements in the analysis of philosophical inquiry, conclusions about that inquiry, and the arguments employed in arriving at these conclusions. It is a little more difficult to make the distinction here than it was in the other two cases. There a distinction could be made between science and the analysis or, as Whewell would say, the philosophy, of it. But in the present case, while we may distinguish conclusions about philosophy from the arguments employed to arrive at those conclusions, nevertheless, both kinds of statement may be said to be philosophical. To distinguish science from a philosophy of science is not particularly difficult. We can see, easily enough, that talk about science is not necessarily science. Talk about physics, for example, is not necessarily physics. But to distinguish philosophy from

the philosophy of philosophy appears, at first view, to be a distinction without a difference. For a philosophy of philosophy is itself philosophy. But I think the distinction can be made significant in the following manner. We can distinguish between the conclusions a philosopher arrives at and the arguments which result in those conclusions. Therefore, in examining the respective analyses of philosophical inquiry conducted by Mill and Whewell, I propose to discuss both the conclusions they arrive at about philosophical inquiry, and the arguments--i.e., the respective investigations--which lead to these conclusions.

Because of the approach which I have taken to the epistemologies of Mill and Whewell, much of the traditional terminology becomes ambiguous. The terms may designate either conclusions or "investigations." Instead of attempting to formulate a new, unambiguous, terminology, I have decided to employ as extensively as possible the traditional terminology ambiguous though it is. By the "traditional terminology" I mean the terminology employed by Mill and Whewell. And, by pointing out the ambiguity in this terminology, I have tried to remove any undesirable feature the ambiguity may have. But occasionally I find it necessary, or desirable, to employ terms which Mill and Whewell do not use. The terms which I employ, and which they do not employ, are discussed in Part B of this introduction.

3. "Analysis of Knowledge" Versus "Epistemology"

I might have entitled my thesis "The Epistemologies of

Mill and Whewell." But, because of the twofold investigation which I wish to conduct, I prefer the title, "The Analysis of Knowledge...." I feel that my orientation towards epistemology is not the usual one. And the latter title states more accurately the project I have undertaken.

4. "Analysis of Knowledge" Versus "Theory of Knowledge"

The expression "The Theory of Knowledge," might also make an acceptable title for the thesis. But I wish to use "theory of knowledge" in a special way. I have, therefore, defined it narrowly and stipulatively so that it has a meaning other than the one generally assigned to it in dictionaries of philosophy. To use "theory of knowledge" in the title would, then, be misleading. For the thesis is about more than theory of knowledge in the sense in which I use "theory of knowledge."

5. "Analysis of Knowledge" Versus "Philosophy of Science"

A great deal of the thesis deals with what might properly be called "philosophy of science." Indeed, if "philosophy of science" were understood in the way in which Whewell understands it, then "The Philosophy of Science according to Mill and Whewell" would make a most accurate title. The philosophy of science, according to Whewell, would, if perfected, be "a complete insight into the essence and conditions of all real knowledge and an exposition of the best methods for the discovery of new truths."[2]

There would be three objections to using "philosophy of

science" in the title of the thesis. (a) "Philosophy of science" does not mean to most people today what it meant to Whewell. (b) In the thesis I deal with a number of topics which are not considered today to belong to the philosophy of science. (c) While Whewell is explicit in declaring that he has a philosophy of science, and in stating what his philosophy of science is, Mill is much less explicit. From one or two remarks which he makes we may infer that he regarded the *Logic* as a contribution to the "science of science." But to what extent he regarded the science of science as a part of science, and to what extent he regarded it as knowledge about science, is by no means clear. There is no doubt in my mind that Mill has a philosophy of science. But it is necessary to recognize that in attempting to set forth what might be called Mill's philosophy of science, I have gone considerably beyond what he actually says on the subject.

6. The Major Subdivisions of the Thesis

In the thesis I attempt to carry out two main purposes, exposition and comparison. A third, but, from the standpoint of the thesis, a minor, purpose is to evaluate. The major subdivisions of the thesis are designed to facilitate the accomplishment of these purposes. Leaving aside the "Introduction" and "Conclusions," the major subdivisions are "The General Character of Knowledge," "The Data of Knowledge," and "The Methods of Knowing." I wish to make a few general comments about each of these items.

(a) The General Character of Knowledge

The general character of knowledge is a topic to which I devote a good deal of attention. Here I try to establish the general orientation of the writers to their subject. What are their biases? What are their preconceived notions? What are they trying to do in the books they write? What, in their opinion, does it mean to know? What are their views on the origin, structure, criteria, nature, grounds and limits of knowledge? Why do they hold these views? And so on. Or, to state the case somewhat differently, if I were not hesitant about identifying a particular philosopher with a particular "ism," I might say that in the sections entitled "The General Character of Knowledge" I attempt to set forth that material which distinguishes Whewell the neokantian and neoplatonist, from Mill the empiricist and positivist. But I have not made a great deal of use of this distinction. So I shall say that what I have done in the sections entitled "The General Character of Knowledge" is to present the material which would be pertinent if one were inclined to work out the "ism" which each man represents.

(b) The Data of Knowledge

The most obvious meaning of "data" would seem to be "the given." But a little reflection makes clear that it is not at all obvious what is "given." Nor is it obvious what it means "to be given." I have, therefore, decided to stipulate a meaning for "data." I shall mean by it "that which is amenable to inference." There are some very complicated problems involved here, and for the full working out of them I

shall have to refer the reader to my two chapters on the data of knowledge. But I should like to state now the major consideration determining the above stipulative definition of "data." The major consideration is that I want a term which will designate any of the following: (1) the conscious states--sensations, affections, and volitions--which, according to Mill, are prelogical and preinferential; (2) what Mill calls "objects," all of which are the result of inference, and all of which are, theoretically, amenable to further inference; (3) what Whewell calls "facts," some of which are the result of inference, and all of which are, theoretically, amenable to further inference; and (4) the statements, or premises, from which deduction proceeds. Lewis Carroll might think that I ought to pay "data" very well, if it is to do all this work. But I think that the definition of "data" which I have offered--"that which is amenable to inference"--makes it possible to use "data" unambiguously. Admittedly, there is some ambiguity in the phrase "amenable to inference." But I do not think that it necessarily follows that "data" is, therefore, ambiguous. If anything is amenable to inference of any kind, then it is a datum.

(c) The Methods of Knowing

I discuss the methods of knowing under three headings: induction, deduction, and intuition. I see no need to state the nature and role of these methods at this point. But I should perhaps acknowledge an assumption about these methods which is implicit in much of what I say about them. The as-

sumption is that any method of knowing is ultimately reducible to one, or some combination of, induction, deduction, and intuition. If it were not for this assumption I should have to entitle my discussion here "methods of knowing" rather than "_the_ methods of knowing."

Such, then, in very general terms is the content of my thesis. It contains an exposition, comparison, and evaluation of Mill's and Whewell's analyses of the data, the methods, and the general nature of knowledge. I wish now to discuss some of the techniques which I have employed in presenting this material.

B. Some Techniques Employed in this Thesis

Where Mill and Whewell have conducted analyses of knowledge, I have conducted an analysis of their analyses. And I have found it convenient to employ certain methods in addition to theirs.

Mill and Whewell, at first view, appear to be expounding and defending strongly opposed opinions. They themselves thought that their opinions on many points were far apart. And their contemporaries agreed that such was the case. One of my motives in writing this thesis is to examine this belief. Are Mill and Whewell actually supporting two distinct epistemologies? If so, what are the distinguishing features of each? And what are the strengths and weaknesses of each? Are Mill's arguments sufficiently coercive that one might become a disciple of his? Or does the advantage lie with Whewell? Are both partly in the right, and partly in the

wrong? Do Mill and Whewell only <u>think</u> they are holding opposed views when, in fact, it can be shown that their views are in agreement? These are some of the questions which I hope to answer. In order to answer these questions I need a technique for arriving at the answers. The technique which I finally arrived at includes the employment of a number of terms, all of them borrowed from contemporary writers. I wish now to make clear six of these terms. They are "orders of knowledge," "metalanguages," "object language," "decisions," "theory of knowledge," and "metatheory of knowledge." I do not make a great deal of use of these terms. Instead I try, to whatever extent I can, to present the philosophers' views in their own terms. But occasionally I find the vocabularies of Mill and Whewell to be deficient for my purposes. On such occasions I employ the expressions listed above. With the possible exception of "orders of knowledge" I do not use these expressions in the manner in which they are generally employed. I should like now to state the way in which I do employ them.

1. <u>Orders of Knowledge</u>

In their writings which are of interest to me in the present thesis, Mill and Whewell take knowledge as one of their subject matters. What they say about knowledge is held by them to be knowledge of knowledge. In my thesis I take what they regard as knowledge of knowledge as my subject matter, thereby attempting to provide knowledge of knowledge of knowledge. If carried very far this terminology becomes very cumbersome. I, therefore, sometimes prefer to use the expres-

sions "first order knowledge," "second order knowledge," "third order knowledge," and so on. It is obvious that the number of orders can be extended indefinitely.

There is little need to elaborate further the device of orders of knowledge. Either this device, or a variant of it, is encountered in a number of contemporary writers.[3]

Whewell can be expounded fairly easily without resorting to this device. For, Whewell himself makes explicit a distinction between science and the philosophy of science. But, where Mill is concerned, the device is sometimes helpful. It is difficult to know, for example, the relation which Mill thought his Logic had to the empirical and formal sciences. But one can show that Mill thought there were orders of knowledge, with order x providing the subject matter of order $x + 1$, order $x + 1$ providing the subject matter of order $x + 2$, and so on. One can show that Whewell subscribes to a similar hierarchy. Having done this one can then set the two systems up in such a way that they can be compared. This is, on occasion, no small gain. For, the vocabularies of Mill and Whewell differ sufficiently that comparison in terms of those vocabularies is sometimes difficult. One is reluctant to restate a number of Mill's statements in the language employed by Whewell, and *vice versa*. The device of orders of knowledge helps to overcome this terminological difficulty.

2. Metalanguages

The term "metalanguage" is also a familiar bit of jargon.

But since I use it in a fashion to suit myself I wish to make my usage clear.

Each of the orders of knowledge, suggested above, has a language associated with it. First order knowledge has a language peculiar to itself, and so on. I shall call the language of second order knowledge a metalanguage, meaning thereby that it is a language designed for talking about first order <u>knowledge</u>. The modern logicians whom I have read mean by a metalanguage a language designed for talking about another <u>language</u>. But I plan to mean by a "metalanguage" a language for talking about a lower order of knowledge. I should like to illustrate this distinction.

For the sake of example, let us consider the statement, "Some Holstein cows are white." Let us say that the item of knowledge, "Some Holstein cows are white," belongs to the order x. For me, where this thesis is concerned, "the order x" signifies an order of knowledge. For the logician it signifies an order of language. The logician's usage here is not very helpful to me because Mill and Whewell considered that they were talking about knowledge, rather than about language. (But, of course the knowledge they were examining was, or usually could be, expressed linguistically. Furthermore, Mill, especially, was very much aware that many of the "problems" of knowledge are closely associated with "problems" of language.)

3. <u>Object Language</u>

In addition to borrowing the logician's term "metalan-

guage," I should also like to borrow his term "object language." But, again, I shall not mean by it what he does. For the logician, an object language is any language which he makes the object of his inquiry.[4] But I shall mean by an object language a language which expresses our knowledge of objects. Thus, for me, the statement given above, "Some Holstein cows are white," is a statement in an object language regardless of whether or not I make the statement itself an "object of inquiry." It is a statement belonging to an object language because it expresses knowledge of things--Holsteins--which are perceived as objects. In relation to the above knowledge about Holsteins, I shall say that the statement, "A cow is an object" can belong to a metalanguage. I shall call this a metalanguage statement when it expresses an item of knowledge of the order $x + 1$ about an item of knowledge of the order x.

There are two main reasons why I wish to use "object language" in the manner set forth above. (1) For both Mill and Whewell, all "important knowledge" (the phrase is employed by both Mill and Whewell) is knowledge of objects. And one of the things both Mill and Whewell attempt to do is give us a better understanding of that knowledge. The statements other than their own writings which most interest them, therefore, are statements which make up what I have called an "object language." (2) Their talk about our knowledge of objects is expressed in what I have called a metalanguage. And my meaning of "metalanguage" is derivative from my meaning of "object language." A metalanguage is a language for talking

about that knowledge which is expressed in an object language. In talking about first order knowledge, _i.e._, knowledge of objects, the metalanguage does not constitute but, rather, gives expression to, a second order of knowledge. Second order knowledge, like first order knowledge, ultimately has nature and experience as its subject matters. But it deals with nature and experience at one remove from them.

I anticipate one objection to the above usage. I have said that "Some Holstein cows are white" is an object language statement, and that "A cow is an object" can be a metalanguage statement. And it will be objected that the latter statement is of the same order as the first. The chief evidence that it is of the same order is that "object" can be defined denotatively. Since "object" can be defined denotatively, therefore, it will be said that, "A cow is an object," belongs to the object language. In answer to the above objection, I offer the following considerations.

(1) As far as common sense realism is concerned there is no point in trying to establish that there can be anything "peculiar" about the statement, "A cow is an object." But some philosophers, like Mill and Whewell, do find something peculiar--_i.e._, distinctive--about such a statement. Mill, for example, would hold that the grounds for asserting that, "Some Holsteins are white," are different from the grounds for asserting that, "A cow is an object." That some Holsteins are white is a deliverance of induction. And the proof of the statement is, finally, simple enumeration. But to establish that a cow is an object, simple enumeration will not suffice.

To establish that a cow is an object Mill has to appeal to what seems to some of his critics to be metaphysics. He has, first of all, to distinguish sensations from objects. Then he has to give reasons, rather than cite inductions, to establish his claim that a cow is an object, not a sensation.

For Whewell, "object" means, among other things, "that which is perceived as being not subject." "A cow is an object," therefore, expresses a knowledge of subjects and objects, not a knowledge of cows as cows. And this (philosophical) knowledge of subjects and objects is not first order knowledge. There is, of course, first order knowledge about subjects and objects. On the first order level we have sciences of objects and sciences of subjects. But on the second order level we know that the radical distinction which first order knowledge makes here is not supportable. It is not supportable because when we come into possession of second order knowledge we then know that the subject-object distinction is not absolute. Subjects and objects are not "cut off from each other with a hatchet," as they appear to be to the person who possesses only first order knowledge. Therefore, for Whewell, the significance of the statement, "A cow is an object," is wholly apparent only to the possessor of second order knowledge. To one who possesses such knowledge the statement is seen to signify that cows are among those things--objects--which are perceived as existing independently of us in space and time. But the philosopher knows that objects owe many of the characteristics which they possess to the sub-

ject which perceives them. For Whewell, therefore, "A cow is an object" is a statement which can belong either to science or to the philosophy of science. But the meaning of "object" is different in each case. As a statement belonging to science it represents the scientific attempt to classify the objects of experience. Objects can be divided into many classes, of which cows is one. As a statement belonging to the philosophy of science it represents the philosopher's knowledge that there are always within knowledge antithetical elements. And one way to express the antithesis is to refer to it as the subject-object antithesis. But the scientist, qua scientist, has no knowledge of this antithesis. "A cow is an object" has there-fore, a special significance for the philosopher. It is like a first order statement in that it expresses a knowledge of nature and experience. But it is really a second order statement because it deals directly with science. And it is only by "carrying on through" science, by reinterpreting and explaining it, that it has any claim to being a statement about nature and experience. Science deals with objects as perceived, and relates them. Philosophy makes clear that the same acts which give us science also give us the objects which science perceives and relates.

(2) Unless it be granted that "A cow is an object" does differ in some radical way from "Some Holsteins are white," then much of what has traditionally passed as philosophy will have to be written off as of no interest. Many of the philosophical arguments which have lasted through the centuries

seem to have arisen out of such observations as this: one might point to the same Holstein in order to define denotatively the three terms "object," "cow," and "white." Now, unless it be allowed that it is a meaningful question to ask, Do these three names, in the above circumstance, denote the same thing? most of Mill's Logic might as well be fed to the flames. For, unless one is willing to grant that an object can be distinguished from a color, then he will certainly yawn over much of Mill's Logic.

(3) As stated above, for both Mill and Whewell, second order knowledge, and higher orders of knowledge, ultimately relate to nature and experience. If they did not, Mill and Whewell would have no interest in them. Therefore, if I am to use the term "metalanguage" in my expositions of Mill and Whewell I have to use it in such a way that it refers to knowledge of nature and experience. A metalanguage can refer to knowledge of nature and experience only if the language which it is about either refers to a knowledge of nature and experience, or expresses a knowledge of nature and experience. Ultimately, therefore, there must be a knowledge which expresses a knowledge of nature and experience. For Mill and Whewell most of our knowledge of nature and experience is knowledge of objects. I have, therefore, in the case of Mill and Whewell, called that language which expresses our knowledge of nature and experience, an object language.

4. Decisions

(a) The Nature of Epistemological Decisions

Morton White states that "the root epistemological problem is that of firm belief."[5] It is my view that neither the device of orders of knowledge, nor the device of metalanguage and object language, enables us to inquire profitably into the nature and grounds of our firm epistemological beliefs. In order to conduct such an inquiry Mill and Whewell resort to various devices. What they are inquiring into in such cases, and what supports their final conclusions in such cases, I shall call "decisions."

To exemplify what I mean by decisions I shall discuss briefly the matter of establishing the conditions under which a statement can be said to be true or false. The truth conditions of a language, it is agreed by modern logicians, have to be stated in a metalanguage. The truth conditions of the metalanguage have to be stated in a meta-metalanguage. And so on. I have no desire to show that there is any flaw in the above analysis. But I do wish to make three observations which are relevant to it.

(1) It is generally held today that first order knowledge (science) is about nature and experience. Second order knowledge is about first order knowledge. And second order knowledge is about first order knowledge in the same way in which first order knowledge is about nature and experience. Second order knowledge takes first order knowledge as its

subject matter in the same way in which first order knowledge takes nature and experience as its subject matters. In other words, there is an analogy here. The analogy may or may not be informative. This question does not concern me at the moment. The question which interests me is this. If we grant the analogy, what have we granted about second order knowledge? Obviously the answer to this question can only be decided after we have determined the way in which first order knowledge is about nature and experience, and here we encounter two different viewpoints.

The modern viewpoint is that first order knowledge is descriptive of nature and experience, and that second order knowledge is descriptive of first order knowledge. Mill's view is not too different from the modern view. His view differs mainly in two respects. (a) For him second order knowledge is about nature and experience in addition to being about first order knowledge. For example, it is an item of second order knowledge that all first order knowledge is inductive. But it also is an item of second order knowledge that nature and experience are so constituted that they are amenable to induction. And (b) some items of second order knowledge which a modern logician would not allow to have self reference, are considered by Mill to have self reference. "All generalized knowledge is inductive," provides an example.

Whewell's view here is distinct from that of Mill's in that, for Whewell, neither first order knowledge, nor any

other order of knowledge, is merely descriptive. Knowledge, at all levels, has the characteristics of being universal and necessary. If it lacks these characteristics it is, by definition, not "really" knowledge. The characteristics of universality and necessity, Whewell argues, cannot be mere descriptions of nature and experience. Instead they are conditions of knowledge. First order knowledge, then, for him, is not a mere description of nature and experience. It is, instead, a relating of perceptions (and, perhaps, of sensations) in such a way as to satisfy certain universal and necessary conditions. And second order knowledge is not a mere description of first order knowledge. It is, instead, an attempt to show the grounds of the possibility of a knowledge which is universal and necessary.

For Whewell, first order knowledge merits the name "science" when it can be shown that its statements are not mere empirical generalizations which have as their ultimate ground induction by enumeration, but are, instead, universal and necessary statements. Whewell means by "universal" here that there are no exceptions to them, and by "necessary" that their contradictories are untenable. Whewell says that a statement is necessary when its contradictory is <u>inconceivable</u>. But "inconceivable" has certain overtones, primarily psychological, which Mill seizes upon to confuse the issue here. Perhaps "untenable" has similar overtones. If so, I have not improved upon Whewell's formulation. But, at any rate, what Whewell intends to assert is that one cannot mean-

ingfully predicate "true" of two contradictory statements. If one is true, the other cannot be. For him, the task of first order knowledge is to establish statements--e.g. Newton's laws of motion--which are seen to hold universally and whose contradictories are necessarily false. Since the contradictories are necessarily false, the statements which assert the laws of motion are necessarily true. And the task of second order knowledge is to make universal and necessary statements about first order knowledge. This it does by exhibiting the *a priori* ground of first order knowledge. (I take it that this is not too far from what a modern logician means when he says that a metalanguage states the truth conditions of an object language.) Since first order knowledge is science by virtue of the *a priori* element, he will not allow that science is merely descriptive. And since second order knowledge in its turn rests on *a priori* grounds, it is not merely descriptive either.

(2) Modern logicians--at least, the ones whom I have read--hold that the truth conditions of an object language have to be stated in a metalanguage, and that the truth conditions of the metalanguage have to be stated in a meta-metalanguage. As a result, one does not find the truth conditions of a language stated within that language itself. But it is conceivable that the truth conditions of the object language, as stated in a metalanguage, and the truth conditions of the metalanguage, as stated in a meta-metalanguage, might overlap. That is to say, the two sets of truth conditions might have

certain items in common. For example, both languages might employ, as intuitively valid, the argument form _modus ponens_. Or, to take an example discussed by both Mill and Whewell--the principle of non-contradiction--both languages might employ the principle that of two contradictories, only one can be meaningfully asserted as true. From a study of a meta-metalanguage, then, one may discover that at least one of the conditions of the truth of metalanguage statements is the same as at least one of the conditions of the truth of natural language statements. One could know, therefore, that at least one of the conditions which is laid down in the metalanguage relative to the object language also holds true of the metalanguage. There does, then, seem to be a sense in which a language--in this case, a metalanguage--could state at least one of its own truth conditions. The devices of orders of predication, and of metalanguages, obviously delimit only what we can _prove_ to be true. They do not delimit what is _known_ to be true. If _modus ponens_ is employed as a valid argument form in an object language, it is a valid argument form. And by resorting to another order of predication and of language, one can "prove" that it is. If this be granted--and I think it must be--then Mill and Whewell are at least not attempting the impossible when they seek the grounds of the validity of statements. But the grounds they assert are probably not of much interest to a modern logician. For they are set forth as belonging to the same language as the language of which they are the grounds. Or, more frequently,

they are "discovered" in "reality" not in "another language."

(3) Relative to the language which relies on them, the statements which assert the truth conditions of a language are always arbitrary statements and have the quality which Morton White calls the quality of being "pinned down." To "pin down" certain statements, and not others, is arbitrary in that, within limits, we can "pin down" any statement we choose. The major limitation on this process is whether or not, having employed the "pinned down" statement as true we end up contradicting ourselves. But the determination to avoid contradiction is equally arbitrary. The devices of orders of knowledge and of metalanguages do nothing to remove this arbitrary quality from knowledge. Mill and Whewell, in appealing ultimately to intuition, are trying to remove the arbitrary quality. But their appeal to intuition is equally arbitrary. What we designate "knowledge," then, is grounded on what I call "decisions." And it is as much a decision to accept certain "pinned down" statements as it is to decide to trust intuition.

I wish now to call attention to three characteristics of epistemological decisions. (1) There is some order of knowledge in which they are accepted without argument. Instead of first being proven, they are put forth on a "let it be granted" basis as axioms, or postulates, or definitions. (2) They function prescriptively. That is to say, they determine the formal nature of knowledge. They not only assert what is true. They determine what is necessarily true. And (3)

they are frequently accepted unconsciously. It is possible to establish what they are. But we rarely attempt to discover what they are. It is only when a writer is interested in setting forth the formal quality of his knowledge that he takes an interest in what I have called decisions. The decisions are then set down as axioms, postulates, definitions, and the like. But generally we do not bother to set them down. And we are frequently unaware that we are committed to them. As Socrates points out in the Meno we frequently "know" many things which we are not aware that we know. The truth of the statements we accept as true implies the truth of other statements. But most people do not bother to figure out what those "other statements" are. And when we do figure out some of them we find other statements "lying beyond." There are, therefore, always statements upon which we act as if they were true, but whose truth we have not established.

The decisions which I discuss fall into two groups. I have called one group decisions belonging to theory of knowledge. And I have called the other group decisions belonging to metatheory of knowledge. I have borrowed these two expressions from Ducasse.[6] But I do not use them as Ducasse does. I see no reason to offer an exposition of the manner in which Ducasse uses these expressions. And I see no need to indicate the manner in which my usage differs from his. It will be sufficient to make my own usage clear.

(b) Decisions in Theory of Knowledge

In any fully developed science, that is to say, in any

science in which the material can be presented in a deductive form, certain primitive statements appear. Acceptance of these primitive statements implies what I mean by a theory of knowledge. For example, Euclid's geometry is formulated by him in terms of certain well known axioms, definitions, and postulates. Later writers have shown that Euclid's set of axioms, definitions, and postulates is incomplete. But, for my present purposes, this incompleteness is of no great importance, since a complete set can be worked out. Whether or not the set is complete is determined by the possibility of deriving the rest of the system from it. That is to say, it is determined by whether or not the axioms, postulates and definitions are sufficient to enable the geometer to prove the Euclidean theorems and solve the Euclidean "problems." I say that these primitive assertions collectively imply a theory of geometry because they imply that geometry is of such a nature that the theorems "follow from" the axioms, postulates, and definitions.

I should also like to point out that the axioms, postulates, and definitions of Euclid have the three characteristics which I assigned to decisions. (1) They are either accepted or not accepted and, in either case, without argument. (2) They function prescriptively. (3) Many persons accept them "without knowing" that they do. A carpenter "employs" the theorems of Euclid every time he cuts a rafter. There are a great many carpenters who can cut rafters. But not all of them could state the axioms, definitions, and postulates

which establish the Euclidean theorems.

Mill and Whewell recognize what they call "conditions" of knowledge, and their "conditions" seem to me the equivalent of my "decisions." For example, Mill recognizes that there are certain propositions which are peculiar where the matter of their truth is concerned. Such propositions, which I have called "decisions," Mill calls "conditions." He says, *e.g.*, that one of the things which makes arithmetic possible is the condition that '1 = 1.'[7] "Let this be doubtful," he says, "and not one of the propositions of arithmetic will hold true."[8] The point I wish to make here is that even though Mill feels that '1 = 1' is capable of being proven to be true or false, it is clear that where the arithmetician is concerned any inquiry into the truth or falsity of '1 = 1' is unprofitable. In relation to the propositions of arithmetic, it cannot be doubtful that '1 = 1.' It is not merely the case that '1 = 1.' Instead, it is the case that 1 must equal 1. The philosopher, then, according to Mill, may raise the issue of the truth of '1 = 1.' But the arithmetician cannot, because for the arithmetician that '1 = 1' cannot be doubted. For him, 1 must equal 1. "In all propositions concerning numbers, a condition--[1 = 1]--is implied, without which none of them could be true."[9]

However, while there is a term--"conditions"--in the writings of Mill and Whewell which I could employ to say what I want to say in this area, I prefer the term "decisions". This is primarily because knowledge, for me, has more of an "if...then" quality than it has for Mill and Whewell. But,

regardless of which term be employed, the important thing at the moment to stress is that I think there are certain statements which are primitive and prescriptive when one comes to state the nature of knowledge. These statements, taken collectively, I call a theory of knowledge. For Mill there are two statements which assert absolutely fundamental conditions, or decisions, and which are, therefore, good examples of what I mean by theory-of-knowledge statements. These are (1) Knowledge is possible if, and only if, there are present to perception particular, self-identical, nameable things. And (2) Knowledge is possible if and only if these particular, self-identical, nameable things can be related.

(c) Metatheory of Knowledge

In order to show the possibility of a science, it is, then, necessary to state its conditions, i.e., the decisions which, when accepted as principles of action, culminate in the given science. Decisions of this order, I have stated above, constitute what I mean by theory of knowledge. Within a given science, these decisions are simply accepted as valid. But it is possible to attempt to show that they are valid. And the attempt to show that they are valid I call metatheory of knowledge.

In metatheory of knowledge we encounter further decisions. *Qua* decisions they do not differ from the decisions which constitute theory of knowledge. That is to say, they have the three characteristics assigned, above, to the decisions constitutive of theory of knowledge. They differ from the decisions constitutive of theory of knowledge only in that they

belong to a different order. Where the decisions of theory of knowledge support the consequence of acting upon the decisions, the decisions of metatheory of knowledge support the decisions of theory of knowledge.

A simple example will suffice to illustrate the distinction between theory of knowledge and metatheory of knowledge. Suppose we hold that science consists in the relating of atomic particulars. This commitment is a decision relating to theory of knowledge. Acting on this decision, we could become practising scientists without ever attempting to defend the decision. But a scientist, or philosopher, <u>might</u> wish to defend this decision. And it is obvious what the defense, in part, must be. In part, the defense must consist in asserting that experience, or nature, or whatever empirical science is said to be "about," does, in fact, provide atomic particulars for the empirical scientist to relate. The atomic character of experience and nature is, then, a condition of the possibility of relating atomic particulars. Therefore <u>in relation to</u> the theory that knowledge consists in the relating of atomic particulars, the assertion of atomic particulars belongs to another order. The order to which it belongs I have called metatheory of knowledge.

It is obvious that the number of metatheories can be extended. For example, one might attempt to support the claim that experience and nature do provide atomic particulars, thereby involving oneself in a meta-metatheory of knowledge.

And in my thesis I occasionally find myself working on this level. But while the above may be an <u>example</u> of different orders, it is not a <u>proof</u> that there are such orders. Furthermore, I do not claim that I can construct a proof that there are such orders. But I can, at least, present the following argument. Mill and Whewell agree that science consists in the relating of particulars. And they further agree (as, indeed, they are obliged to do since they subscribe to the preceding statement) that there are particulars. The two statements differ in at least the following respects: (1) The first is a statement about science, the second, about particulars. (2) The first is a description (and, I think, a prescription) relating to the form of scientific knowledge. The second relates to the content of scientific knowledge. The second requires that we determine what kinds of particulars there are to relate. But the first does not carry with it any such requirement. (3) The second is a ground of the first. But the first is not a ground of the second. It is this third point which interests me most as a possible proof of the statement that epistemological decisions exhibit a hierarchical order. If we are going to accept as a basis for action that scientific knowledge consists in the relating of particulars, we can only justify this decision by the further decision that there are particulars to relate. I propose, therefore, to say that the former statement belongs to the order of theory of knowledge, and that the latter belongs to the order of metatheory of knowledge.

If the above distinction between theory of knowledge and metatheory of knowledge be accepted, then it follows that there could be any number of meta-metatheories of knowledge. For example, if the above statement, "There are particulars," be taken as an example of a statement on the level of metatheory it might reasonably be asked, "What supports the statement that there are particulars?" In attempting to answer such a question we should find ourselves involved in a meta-metatheory. Let us say that we should answer the question by saying that intuition supports the statement about the reality of particulars. One might next reasonably inquire, What supports intuition?

To establish the orders of theory of knowledge one must show what "supports" knowledge at any given level. In order to establish these orders it seems to me best to work one's way "down" the "ladder," not "up." Of the two philosophers whose works constitute the basis of this thesis, Whewell is more inclined to begin with science and work his way "down." Mill is more inclined to begin somewhere "down" the "ladder," and work his way "up" to science. When all is said and done, perhaps it makes very little difference in which direction one chooses to go. But I should like to make one observation on this point. As a result of his attempt to work his way "down" Whewell is much more aware than is Mill that there is no possibility of standing on the "bottom rung" of the "ladder." Unlike a modern logician, Whewell does assert that there <u>must be</u> a "bottom rung." For Whewell, this is the Ab-

solute. But he states that we can never have knowledge of it over and above our knowledge that there must be an Absolute.

I also wish to point out that I have ascribed to Mill and Whewell a number of "decisions" to which they do not explicitly subscribe. But I have tried always to make clear my grounds for saying that implicitly they are subscribing to this or that. Furthermore, I wish to state that I have contributed more to Mill's account of his position than I have to Whewell's account of his. Where Whewell is concerned my major task has been to achieve accurate exposition. But in the case of Mill I have engaged in a considerable amount of speculation in order to present Mill's position in the form in which I wish to present it. Mill deliberately avoids a number of the issues which are of major interest to me.

> The great mass of Mr. Mill's labor has been devoted to what may be termed the middle ground of human thought, below the primary data which reason must assume, and short of the applied science which has practice for its end. At the upper limit shunning the original postulates of all knowledge, and at the lower its concrete results, he has addressed himself to the intermediary processes, and determined the methods for working out derivative but still general truths.[10]

It is what Martineau calls the "upper limit" that is my main interest. And since Mill does not supply "the original postulates of all knowledge," as Martineau calls them, I have attempted to infer what these are in Mill's case. My thesis is not primarily concerned with the middle ground of human thought. But--and this is especially true in Mill's case--I have to set forth the middle ground in order to get at the

"upper limit." As Martineau goes on to say, Mill's discussion of knowledge, especially in the Logic, may be satisfactory as an analysis of method, or as providing a set of rules for the interpretation of phenomena and the discovery of laws. "But considered as a philosophy, giving the ultimate rationale of the intellectual processes it describes, it leaves us, we confess, altogether unsatisfied."[11]

C. Summary

In the above material I have outlined the subject matter of the thesis and certain techniques which I intend to employ. I wish now to review very briefly what I have said on these two topics. I shall begin with what I have said concerning the subject matter of the thesis.

1. I have entitled the thesis "The Analysis of Knowledge in John Stuart Mill and William Whewell." The phrase "the analysis of knowledge," as it is used in this thesis, refers to two quite different things--conclusions about knowledge, and the methods employed to arrive at these conclusions.

The conclusions about knowledge which interest me are conclusions about two different kinds of knowledge--science, and what Whewell calls philosophy of science and Mill calls logic. Mill and Whewell are content, for the most part, to delineate a rationale of science. They state their conclusions concerning the nature of scientific conclusions and the support which scientific conclusions have. A major portion

of the thesis is devoted to expounding, comparing, and evaluating what Mill and Whewell say on these matters.

But, where the interests of Mill and Whewell lie mainly with the conclusions reached in the recognized sciences, my major interest lies in what Whewell calls philosophy of science and in what Mill calls logic. Mill and Whewell, as a result of analyzing science provide, respectively, a logic, and a philosophy of science. But they do not, to any great extent, take the conclusions reached in their own works and investigate the nature and grounds of these conclusions. The conclusions reached in Whewell's Philosophy of the Inductive Sciences and in Mill's Logic support, according to their respective authors, the conclusions reached in science. But a question of interest to me is this: What supports the conclusions reached in the Philosophy of the Inductive Sciences and in the Logic? Since I should like to know the answer to this question, I have attempted to find out what Mill and Whewell say about it. But their accounts of the structure which supports their own works are not as complete as they should be. And where their accounts are deficient, I have attempted to make good the deficiency.

2. Where techniques are concerned, I require techniques which will accomplish three aims-- exposition, comparison, and evaluation. For the most part I have availed myself of the techniques which Mill and Whewell themselves employ. But in addition to these I occasionally employ the following expressions: "orders of knowledge," "metalanguage,"

"object language," "decisions," "theory of knowledge," and "metatheory of knowledge." I wish to restate very briefly what I mean by these expressions.

The phrase "orders of knowledge" expresses my view that the kinds of knowledge can be arranged hierarchically. In my case I mean by this only that one kind of knowledge can be about another kind of knowledge. The basic kind of knowledge, whatever it may be said to be in any particular philosophical system, I call first order knowledge. Knowledge about first order knowledge I call second order knowledge. And so on. It is clear that, theoretically, the number of such orders is infinite.

By a metalanguage I mean a language in which is expressed knowledge of a given order about knowledge of a "lower" order.

By an object language I mean a language which expresses our knowledge of objects.

By decisions I mean statements upon which we are willing to act as if they were true, but whose truth we have not established. Such statements function prescriptively to the extent that they determine the formal nature of knowledge. They are frequently accepted unconsciously. And when accepted consciously it is not usual to ask for proof of their validity.

By a theory of knowledge I mean certain decisions which, collectively, form the basis of our knowledge systems. I do not include under a theory of knowledge the axioms and postulates of the recognized sciences. Instead I mean by a theory

of knowledge a set of statements which are more primitive than those which appear as axioms and postulates within the recognized sciences. If there are such statements--and I try to show that there are--then we may speak of a theory of geometry, a theory of physics, and so on. But primarily I mean by a theory of knowledge a set of statements which underlie <u>all</u> the sciences. These statements, if there be such--and I contend that there are--truly give us a theory of knowledge, as opposed to a theory of geometry, a theory of physics, and the like. As an example of a statement which truly belongs to theory of knowledge I shall cite the statement that knowledge consists in the relating of particulars. Such a statement could, of course, properly be included among the axioms, or postulates, of geometry, physics, and the rest. But in the geometry books, physics books, and so on, which I have read, it is not included.

By a metatheory of knowledge I mean decisions which support a theory of knowledge. If the statement, "Knowledge consists in the relating of particulars" belongs to a theory of knowledge, then in a metatheory which supports this theory, the statement, "There are particulars," will have to appear. The latter statement is a ground of the former, and so belongs to a different order. Theoretically, the number of metatheories of knowledge is infinite.

3. The following thesis, then, consists of an elaboration of the subject matter set forth in 1, above. In order to elaborate the subject matter set forth in 1, above, I have

employed the techniques employed by Mill and Whewell, and also the techniques outlined in a, above.

I first attempt to provide an exposition of the views about science, and the views about the logic, and philosophy, of science provided by Mill and Whewell. I then compare and evaluate the two views, and arrive at eleven conclusions which are stated in the final chapter. In order to facilitate exposition, comparison, and evaluation, I discuss the views of Mill and Whewell under three main headings--"The General Character of Knowledge," "The Data of Knowledge," and the "Methods of Knowing." Under "The General Character of Knowledge," I discuss the writers' orientation to their subject. Under "The Data of Knowledge" I discuss whatever Mill and Whewell find to be amenable to inference. And under "The Methods of Knowing" I discuss their views on induction, deduction, and intuition.

CHAPTER II

THE GENERAL CHARACTER OF KNOWLEDGE ACCORDING TO WHEWELL

In this chapter I wish to set forth the major theses, relative to knowledge, which Whewell supports. I also wish to give some indication of the arguments which support these theses. I deal with all of these items in a more elaborate way in subsequent chapters. But there is some value in giving a synoptic view before attempting to give a detailed exposition. I shall attempt to give this synoptic view by dealing with the following topics: 1. Physics as the paradigm of science; 2. The fundamental antithesis; 3. The subjects which perceive; 4. The forms of perception and of knowledge; 5. The objects of perception; 6. Whewell's eclecticism; 7. The relation of epistemology to ontology; 8. Whewell's realism and his employment of the correspondence theory of truth; 9. The methods of knowing; 10. Philosophy and its relation to the other kinds of knowledge. By means of these ten topics I can state those doctrines of Whewell's which, in my opinion, are basic.

1. Physics as the Paradigm of Science

There are two major reasons according to Whewell why physics is the paradigm of science. The first is that physics

gives us a science of the objects of experience. The second is that physical knowledge can be set forth in the deductive form which we find in Euclid's *Elements*.

Whewell's inquiry into knowledge presupposes that the knowledge we are primarily interested in is knowledge of the objects of experience, and that we already possess instances of such knowledge.

Knowledge of the objects of experience Whewell calls "important" knowledge. It is important because it is knowledge of the objects of experience. "Everyone knows" that by "important knowledge," in the full sense of that phrase, is meant knowledge of the objects of experience. It is also important because knowledge is "power" to control and manipulate the objects of experience. But Whewell, and, according to Whewell, the scientist, have no interest in knowledge as power. He, and the scientist, are interested only in knowledge *per se*.

That we possess knowledge of the objects of experience is known to anyone who knows anything. For knowledge, even of the "lowest" kind, is necessarily knowledge of the objects of experience. Knowledge of this lowest kind is not science. But it and science are alike in two respects. They have the same subject matter, and each is the result of uniting a non-cognitive manifold by means of acts of the intellect.

But while they are alike in the above two respects, knowledge of the lowest kind, and science differ in two respects. Knowledge of the lowest kind consists in "particular" statements, *i.e.*, in "here-now" statements. Science contains many

such statements, but one of the things which characterizes it as science is that it also contains many "general" statements, statements which do not have to be qualified by the phrase "here-now." Knowledge of the lowest kind, and science, are both the result of uniting a non-cognitive manifold by means of acts of the intellect. (No mere pre-cognitive "awareness," qualifies as knowledge in Whewell's analysis). In pre-scientific knowledge the uniting of the manifold by acts of the intellect is unconscious. But in science it is conscious. The scientist cannot bring all the acts of the intellect into consciousness. Objects, for example, which the philosopher knows to be "constructed," are simply "given" to experience whether the experience be that of Newton or of some uncultured savage. In this area, while there is both a <u>natura naturans</u> and a <u>natura naturata</u>, <u>natura naturans</u>, where objects are concerned, is beyond recall. We can have a theory, or, as Whewell would say, a philosophy, about it. But not a science of it. The science we can have relative to objects deals with their relations, and these the physicist can bring into consciousness. He can state, in a fashion peculiar to himself, those relations which we are now conscious of, and he can discover, and thereby bring into consciousness, many relations of which we are presently unaware.

Objects are perceived <u>as</u> related, known <u>as</u> related. As stated above, one distinction between science and the lowest kind of knowledge, is that science makes explicit those relations which even the lowest kind of knowledge exhibits.

These relations we may call the most obvious relations, meaning thereby primarily that everyone perceives objects as so related. The most obvious relations are spatial, temporal, and numerical ones. These are relations which are absolutely necessary for perception. Without these we should not perceive objects at all. Primarily because of the obviousness of these relations, we now have sciences of them, whereas we do not have sciences of the less obvious relations. (I shall try to make clear later what science is. All that I want to establish at the moment is that physics is the paradigm of science). These three relations give rise to at least four sciences. There is a science of space. There is a science of time.[12] There is a science of number. And there is a fourth, physics, which employs the above three to give us a science of objects. The first three are interesting, and are useful in ways which I shall describe later. But it is the fourth, physics, which is important. It is important because it gives us the kind of knowledge which we most want. The first three give us sciences of relations _per se_. But these sciences themselves are not sciences of the relations among _objects_. Physics is a science of the relations among objects, and this is one of the reasons that it is the paradigm of science.

The second major reason why physics is the paradigm of science is because it has achieved the universality and the necessity characteristic of, for example, Euclidean geometry. (Euclidean geometry, for Whewell, is the science which treats

of space as a form of perception.) Physics has achieved the universality and necessity characteristic of geometry by taking on the form of geometry. Our physical knowledge can be set forth in the form employed by Euclid.

It may now seem that we have made geometry, rather than physics, the paradigm of science. A science "becomes" a science when it takes on the form exhibited by geometry. Such, however, is not the case. The possession of the geometric form is only one of the criteria of a science. Another criterion, which geometry lacks, is that a science must relate the objects of perception. And geometry, *per se*, does not do this. Astronomy does it, often by the employment of geometrical conceptions. An example of this is to be found in the Keplerian laws of the planetary orbits. Physics does it, as, for example, in the Newtonian laws of motion. And the elaboration and implementation of the laws of motion frequently requires the employment of geometrical conceptions. But geometry in and of itself does not do it. In the final sense of the term "science," then, physics and astronomy are sciences. But geometry is not. And the physicist and the astronomer are scientists--a term brought into popular use by Whewell--but the geometrician is not.

Whewell does not, however, consistently hold to the above distinction. He refers a great many times to the science of geometry. It is, therefore, necessary to qualify the above distinction and say that physics is the paradigm of the *kind*

of science which we most want. Even so, the point made above is worth repeating. The physicist is a scientist. The geometer is not.

2. The Fundamental Antithesis

The theme which occurs most often in Whewell's *Philosophy of the Inductive Sciences* is that within knowledge, *i.e.*, within that kind of knowledge which the philosophy of science takes as its subject matter, antithetical elements appear. (Whewell neglects to state whether or not these antithetical elements are also present in his *Philosophy of Science*.) The presence of these antithetical elements is not necessarily the definitive characteristic of knowledge. But it is the characteristic which is the most informative. And to familiarize himself with this characteristic is the most profitable inquiry into knowledge that the philosopher of science can undertake. Both the knowledge which we call science, and prescientific knowledge, exhibit these opposed, or antithetical elements. Whewell's subject matter is almost solely science, inductive and deductive. And what he attempts to do in his statement of the general character of knowledge, is to exhibit the presence, within the recognized sciences, of what he calls "fundamental antitheses." Frequently, however, he illustrates his exposition by examples taken from common sense knowledge.

These antitheses, Whewell tells us, are really one and the same antithesis "appearing" under various "guises." (To be less metaphorical about it, we could say that we give the antithetical elements different names in different situations.)

The antitheses are numerous. Among those which Whewell lists are the following: thought-thing, subject-object, theory-fact, idea-sensation, the world within - the world without, man-nature, me-not-me, necessary truth - experiential truth, form-matter. In each case one half of the antithesis has an "inner" reference, the other an "outer."

The first important point to note, then, is that there are always present within knowledge opposed elements. The second important point to note is that within knowledge these opposed elements are combined. For knowledge to appear, both elements are necessary. To take one of the above antitheses by way of example, both thoughts and things are necessary if there is to be knowledge. Without things there would be no reality. And without thoughts there would be no connection among things.[13] And without the uniting of the two--i.e. without the uniting of thoughts and things--there would not be knowledge either.

The thought-thing antithesis is not "given." It is not a datum of experience. "Thoughts and things are so intimately combined in our knowledge, that we do not look upon them as distinct. One single act of the mind involves them both; and their contrast disappears in their union."[14] The antithesis becomes apparent only when we reflect upon experience and/or knowledge. (Because of the intimate relation between what we experience and what we know, "knowledge" and "experience" are frequently synonymous terms for Whewell.) It is not the scientist, but the philosopher of science, who discerns the

antithesis:

> Though Knowledge requires the union of these two elements, Philosophy requires the separation of them, in order that the nature and structure of Knowledge may be seen.[15]

Furthermore, philosophy is hard put to accomplish its task of separating these elements. For not only is it the case that these elements cannot be separately exhibited in experience. It is also the case that, strictly speaking, "they cannot be separately conceived and described."[16] It should be noted that Whewell does not assert that they cannot be conceived or described, but that they cannot <u>separately</u> be conceived or described. In order to conceive or describe either we have to have the other also in mind. There are no thoughts which are known purely as thoughts. Our thoughts are always thoughts of things. And there are no things known purely as things. Things are always things-thought-about. All objects are known under a form of perception, and the form of perception is contributed by the knower, not by the object. A known object, therefore, always exhibits ideal as well as objective characteristics.

When we take the whole of science--<u>i.e.</u>, all the sciences--as our subject matter, we note that the fundamental antithesis appears therein in several forms. The sciences may be divided into formal and factual, into necessary and experiential, and into deductive and inductive. Or we may collect these terms and speak of the formal, necessary, and deductive sciences as opposed to the factual, experiential, and inductive sciences. But once again it must be pointed out that the antithesis, al-

though it can be pointed out, is not absolute. The distinction, Whewell would say, is a philosophical distinction. And the philosopher is required both to describe the distinction and to account for it.

The distinction is most easily described by showing that one kind of science is formal, necessary, and deductive, whereas the other is factual, experiential, and inductive. The former are the sciences of what could not be otherwise, such as geometry and arithmetic. The latter are the sciences of what is, but might have been otherwise, such as chemistry and descriptive astronomy. The distinction is most easily accounted for by showing that the one kind of science consists solely of the explication of conceptions, whereas the other consists of the colligation of facts.

There seem to be two main reasons why the forms of perception--space, time, number, causality, and the like--could not be other than they are. The first is that the perceiving mind is, in large measure, definable in terms of these forms of perception. The second is that the nature of perception is tied in with the will of an Author of the universe. It is true that if the forms were to change, then new sciences based on these new forms would arise. But this does not invalidate the claim that the sciences based on the present forms are absolute. And it is inconceivable to us that they should change. But, while the forms are a constant and independent factor, what is perceived under them is dependent upon what is there to be perceived. Furthermore, it is con-

ceivable that the objects of perception could have been otherwise, and may in future change. Therefore, the sciences based upon them lack that independence possessed by the sciences which deal with space, time, and number or, in general, with the formal, or ideal, element of the thing known.

3. The Subjects which Perceive

In the present section I wish to state two interests which Whewell has in the knower as subject. (a) The knower exists as subject, thereby contributing to the furniture of the universe. Since the knower exists, and can be known, Whewell is interested in the kind of knowledge which we can have of the knower. (b) The knower is that which knows. To know means to relate by means of an act of the knower. Therefore, in order to understand knowledge we must understand these acts. Where both (a) and (b) are concerned, Whewell has a major difficulty to overcome. In order to know the knower as knower, the knower, in the act of being known, must not lose its qualities as subject. If it did, then we should know it as object, not as subject. In order to cope with this difficulty Whewell introduces two kinds of knowledge which I wish to mention very briefly in this section. One is introspective knowledge. The other is ethical knowledge. The former is of little interest because Whewell's account of introspection is very vague. The latter is of considerable interest. Through ethical knowledge I know myself as reason, i.e., as the source of universal and necessary laws.

One way to formulate the fundamental antithesis is to express it as the antithesis of subject and object. Not only is there a subject-object antithesis within knowledge. There are also existential subjects and objects. "I" denotes a subject. "Tree" denotes an object. Whewell uses the terms "I", (or "me"), "self," "mind," "knower," to refer to the subject without, apparently, making any radical distinction among these terms. They seem to denote the same thing although they are associated with different expressions of the fundamental antithesis: me-not-me, self-not-self, mind-body, knower-known.

There is no doubt that the subject has an existential status as subject. Whewell says, for example, that if I wish I can make myself, who am a subject, the object of inquiry, and thereby know myself. I take him to mean, in this passage, that I am a possible object of experience. And if I am to know myself as subject, I must as "object known" retain the characteristics which I possess as subject. Whewell advocates introspection as the best device to employ in the search for self-knowledge where our desire is simply to know the nature of that which exists as subject. Introspection is the best method to employ because the facts we are seeking are "internal facts," namely, "our own emotions, thoughts, and springs of action; facts which are connected by ties existing in our own consciousness, and not in mere observed juxtaposition, succession, or similitude."[17] He points out, however, that sometimes in our investigations concerning subjects we

may very properly employ the method of experimental inquiry in addition to the method of introspection. He says that a number of the questions of interest to psychology, anthropology, and political economy can hardly be solved in any other way than by experimental inquiry.[16]

But Whewell is not very much interested in that knowledge of the self, or subject, which goes to make up psychology, anthropology, and political economy. Instead his major interest is in the self as knower. And he concludes that in our attempt to know the self as knower introspection is of little use. Instead, what is required is an examination of the sciences and of the conditions which make the sciences possible. As a result of this examination he concludes that the knowing subject is the source of the Fundamental Ideas which are the ground of the necessity and universality which science exhibits. The knowing subject is also the locus of a number of dispositional powers such as reason and intuition. Whewell claims to know also that the knowing self is something other than these Ideas and powers. But he does not say how he arrives at this knowledge. Furthermore, as to what he knows about this "something other" he says very little. Instead he, for the most part, devotes himself to determining what its functions necessarily are.

The knower, Whewell insists, is both a spectator and an interpreter of nature. The knower has a most important function in Whewell's theory of knowledge because it is via the knower's ideas that reality is known. In terms of Whewell's

metatheory the knower is seen to be intimately related with the known, and makes an active contribution to what is "there" to be known. The knowable is limited to that which can be brought within cognition, _i.e._, can be related _via_ ideas. Pure sensations--if there were any such thing apart from acts of the intellect to bind them together--would come and go in a meaningless fashion. Such sensations would be meaningless because they would be unconnected and uncompared and would vanish from our awareness when they ended. Insofar as "experience" has any meaning, that is to say, insofar as it truly is experience, its meaning is given to it by acts of the knowing subject. The objects, then, which the subject perceives as spectator are objects which he has constructed as knower. For to know is to create, order, classify, simplify, and interpret. And since our powers are limited, our knowledge is similarly limited. Our powers are limited in two ways, both derivative from the fact that they are human powers. Since they are human powers, as opposed to animal, we see things in a peculiarly human way. And, secondly, since they are only human, as opposed to divine, they are limited in the sense of being imperfect and to some extent--although to what extent we, of course, do not know--unequal to any demand we might make for final and absolute knowledge.

In Whewell's ethical writings we find the knower playing, not only an important role, but a predominant role. For here the knower, in very large measure, supplies both the form and the content of ethical knowledge.

Whewell's ethical system is very complex and represents a not entirely successful attempt to integrate many disparate elements into an eclectic whole. To work out this system would be foreign to the purposes of the present thesis. But one or two features of the system seem pertinent to the present inquiry. Whewell recognizes that ethics is not a science, and his observations as to how it might become one throw some light on his epistemological views.

The first interesting thing is the analogy which he draws between ethics and geometry. When ethics becomes a science, it will resemble geometry in that its propositions will be universal and necessary. In order to be universal and necessary they will be ideal rather than empirical in their origin, _i.e._, they will be grounded in Ideas rather than in sensible intuitions.

The second interesting feature is the manner in which ethics is related to our own nature. I think it would be a mistake to hold that for Whewell all the sciences are essentially grounded in our own nature, that we somehow or other elaborate into sciences an innate mental structure, which preexists science. The sciences are not grounded in our nature, but in axioms. And, temporally speaking, we begin not with axioms, and not with an intuitive insight into our Ideas, but with experience, experience to which we, as knowers, actively contribute. Our contribution to a science originally is largely unconscious and only careful and diligent inquiry can reveal to our consciousness what we as active agents have

contributed to experience in formulating a science of it. There was a knowledge of geometry before anyone undertook to formalize it. And although in order to establish the universality and necessity of the truths of geometry it was necessary to work out the axioms appropriate to it, nevertheless some geometrical truths were known prior to a knowledge of the axioms of geometry. The axioms then, have what we may call logical priority, but not a temporal priority. Euclid did not create geometry out of his head but, instead, worked out the axiomatic basis of an already existing body of knowledge, thereby turning that body of knowledge into a science.

So, too, in ethics, although it undoubtedly has an axiomatic basis, it does not follow that we can begin with its axioms. Temporally speaking, a knowledge of its axioms represents a goal rather than a starting point. Ethics as a science, of course, is, or should be, deduced from self-evident axioms, the guarantee of the axioms being to some extent appropriate Ideas. Furthermore, these should be axioms respecting our own nature, especially our moral nature. But this does not mean that we can somehow or other look inside ourselves to discover those axioms. Furthermore, the laws of our own nature as it <u>is</u> constitute psychology and anthropology, not ethics. And, although moral precepts which tell us what our nature <u>ought</u> to be, may be applicable to psychological and anthropological data, they are not derivable from these. We can, however, begin with moral precepts, show that these involve an Idea of morality, and attempt to make that Idea ex-

plicit. When we do this we discover that morality is action in conformity with reason. And when we inquire as to what it means to act in conformity with reason, we discover that it means to act in accordance with a rule, the rule being universal and necessary. Now, reason is a factor which the knower brings to, or contributes to, experience, so that from the standpoint of ethical knowledge the most important role of the knower is to act as reason and thereby to act as the source of moral rules. And it also turns out that this is the most important role of the knower in relation to any other kind of knowledge. This role which the knower plays gives us our most important insight into what the knower *qua* knower is. The knower is the source of universal and necessary rules.[19]

4. The Forms of Perception and of Knowledge

The forms of perception are said by Whewell to be Ideas and Conceptions. Space is an example of an Idea. Triangle is an example of a Conception. Most of Whewell's observations which are of epistemological interest relate to Ideas and Conceptions.

Ideas and Conceptions are antithetical to sensations. And since knowledge is understandable only in terms of the antithetical element which it exhibits, sensations should be of some interest. Nevertheless, Whewell, *qua* epistemologist, seems to have relatively little interest in them. I think there are two main reasons for this. (*a*) Sensations never

appear within perception as sensations. But Ideas and Conceptions do appear as Ideas and Conceptions. We can, therefore, have an intuitive knowledge of Ideas and Conceptions, but not of sensations. And, (b) Whewell is convinced, on grounds which I shall subsequently elaborate, that it is reason, not the senses, which give us knowledge.

Statements about sensations function as principles of explanation. But the explanatory power of such statements is very limited. They explain why the world appears as it does only to the extent that they denote an irrational quality of experience, *i.e.*, the way in which objects appear. It is, then, barely proper to say at all that statements about sensations explain *why* the world appears as it does. Such statements assert only *that* the world appears as coloured, sounding, odoriferous, and so on. A *theory* about sensations, on the other hand, does go some way to explaining why the world appears as it does. An example of a theory about sensations is that sensations are either preceded by, or accompanied by, physiological modifications and adaptations. But such a theory does not explain why we have the sensations which we do have. It does not, for example, explain why we see the sky as blue and grass as green. Furthermore, a theory about sensations is not set forth in terms of sensations, but in terms of Ideas and Conceptions. The senses do make some direct contribution to science. For example, we employ the senses to establish scales whereby to measure intensive magnitudes. But, even here, we do not literally employ sensa-

tions, but perceptions. And the element in perception which is amenable to scientific treatment is the cognitive or, as Whewell calls it, the Ideal element.

The forms of perception and of knowledge, then, are cognitive forms.

5. <u>The Objects of Perception</u>

One formulation of the fundamental antithesis expresses it as the antithesis between thoughts and things. In the present section I wish to state briefly what Whewell means by "things."

"Things" is employed in at least two ways by Whewell. According to one usage, that which can be designated by the demonstrative "this" qualifies as a thing. Hence, "thing" denotes such disparate entities as yellow, a triangle, the Idea of space, and the moon. According to the other usage, "thing" denotes only that which is perceived as having at least spatial, temporal, and numerical properties. Hence, of the entities designated "thing" in the first usage, only the moon would qualify as a thing.

In the first meaning of "thing", the sufficient condition which must be met in order to employ the name "thing," is that that which is so named must be capable of being perceived as itself. It must, as far as perception is concerned, be "what it is and not another thing," to borrow a phrase from Bishop Butler.

Where the second meaning of "thing," is concerned, self-identity is a necessary, but not a sufficient, condition of

its "thinghood." A thing, in the sense of an object--*e.g.*, the moon--must be capable of being perceived as what it is and not another thing. This gives it a numerical property. But in addition to this, it must also be present in space and in time. And it must also itself exhibit spatial and temporal properties. That is to say, it must be extended, and it must endure for a measurable period of time.

6. Whewell's Eclecticism

Whewell has adapted for his own use the ideas of a great many authors. I am interested in the eclectic character of his position to the extent that this eclecticism appears within his philosophy of science. His philosophy of science has undoubtedly been influenced by numerous writers. But I emphasize primarily his indebtedness to two, namely, Plato and Kant. I emphasize these two because they are the most influential in determining his theory and metatheory of knowledge. I see no very good reason to set forth here what I think are all the "Kantian" and "Platonic" elements in his philosophy of science. But I shall point out some of them. The intent of this thesis, where Whewell is concerned, is to set forth his position systematically, not to trace it to its historical origins. But it is in my own interests to point out that Whewell's position is eclectic. For his eclecticism is not entirely successful. And since it is not, difficulties arise in setting forth his position systematically.

Whewell is said, by most authors, to be a Kantian. He

himself denies that he is a Kantian.[20] But it is evident that Kant had a profound influence upon him. Whewell acknowledges that he has borrowed certain ideas from Kant, but denies that he is simply restating the Kantian position. This is, in my view--and my view here is supported by Ducasse[21]--undeniably the case.

Not only does Whewell acknowledge an indebtedness to Kant. He also says that he has been influenced by Fichte, Schelling, and Hegel. The central doctrine in his philosophy of science is, according to Whewell, the doctrine of the fundamental antithesis. This is the doctrine that in every act of knowledge there are opposed elements, the one being Ideal, the other perceptual. And he relates this doctrine to the doctrines of "the Germans" in the following way. From Kant he accepted the view that knowledge derives its form and character--i.e., its universal and necessary quality--from the Ideal element involved in every act of knowledge. From Fichte he derived the view that all knowledge involves a development of the Ideal conditions of knowledge, and that these conditions exist in our own minds. From Schelling he derived the view that although Ideas and perceptions can be distinguished and separated "in our philosophy," they cannot be distinguished and separated "in fact." "In fact" they are simply different aspects of the same thing.

I take it that Whewell means here that they are different aspects of objects. Ideas appear, in the object, as the formal properties of the objects. Sensations appear, in the

object, as whatever qualities may be given to cognition <u>via</u> the senses. The object, for example, is perceived as extended. But, upon reflection, or "in my philosophy," I can distinguish between the perceived extension of the object, and extension <u>per se</u>. The perceived extension is sensible. It is, for example, coloured. The Idea of extension is not coloured, and cannot be sensibly perceived. Instead it has to be conceived. In my philosophy I decide that the reason why the object is perceived as extended is because I have united certain sensations by the employment of the Idea of extension. But in the object the perceived extension, and extension <u>per se</u>, are indistinguishable. I take it that this is what Whewell means when he says that "in fact" they are indistinguishable. "In fact" means "in the object," <u>i.e.</u>, "in the realm of fact." Whether or not this is also Schelling's view, I am not prepared to say. But, at any rate, it is Whewell's opinion that from Schelling he derived the view that Ideas and perceptions "in fact" are but different aspects of the same thing.

From Hegel he derived the view that "the only way in which we can approach the truth is by gradually and successively, in one instance after another . . . [advancing] from the perception to the idea; from the fact to the theory; from the apprehension of truths as actual to the apprehension of them as necessary."[22]

As a result of reflecting upon the above doctrines Whewell formulated his doctrine of the fundamental antithesis. He

also arrived at the conclusions that the progress from fact to theory suggested by Hegel constitutes the history of science, and that this progress

> although always leading us nearer to that central unity of which both the idea and the fact are emanations, can never lead us to that point, nor to any measurable proximity to it, or any definite comprehension of its place and nature. [23]

These Hegelian doctrines--if they be such--do play some role in Whewell's philosophy of science. But it is primarily to Kant that Whewell is indebted.

The other major source of inspiration for Whewell's philosophy of science is Plato. The "Platonic" element appears primarily in the form of the doctrines that Ideas are archetypes which exist prior to human experience, and that these Ideas have an eternal existence as Ideas in the Divine Mind. (This is, I should think, a neoplatonic, rather than a Platonic, view). The universe has been constructed by a Creator after the pattern of the Ideas. The universe, therefore, has, from the point of view of its observer, a pre-existent structure. I, by the employment of my intellect, do not give form to nature. Instead I am presented with a universe which is already formed, and my task is to discover the form which God has given it. Such a view as this is in direct opposition to the view put forth elsewhere that Nature is simply the totality of objects of possible experience, and that I, by the employment of facts of my own intellect, give to the world of perceived objects that order which I subsequently discover in it. I give it this order (1) by imposing upon a

sensible manifold, such Ideas as space, time, and number, and Conceptions derived from these Ideas, and (2) by consciously uniting my various perceptions by means of the above, and other appropriate, Ideas and Conceptions in such a way as to provide myself with the laws and causes of phenomena.

On the "Kantian" view man is an observer of nature, it is true; but he is, primarily, the interpreter of nature. On the "Platonic" view he is an observer merely.[24]

7. The Relation of Epistemology to Ontology

Whewell's epistemology clearly involves an ontology. By an ontology in Whewell's case, I mean, in part, a subscription to at least one substance. And by "substance," I mean, to paraphrase an expression of Descartes', something which could exist if nothing else existed. One thing which, for Whewell, could exist if nothing else existed is God. Sometimes "God" simply denotes a First Cause. In other passages "God" denotes a divine Creator. Something which requires only the existence of God in order to exist itself is mind. Whatever mind is, it is not reducible to anything else. In the case of the human mind, for example, the mind is not the brain and nervous system. Nor is it the activities of these. It is the locus of certain dispositional powers--sagacity, invention, genius, insight, and the like. It is not the sum total of these powers, although that it possesses these powers is almost the sum total of our knowledge of it. But we also know, on the basis of revelation, certain truths concerning mind. And it is inconceivable either that these truths apply merely to the disposi-

tional powers, or to the brain and nervous system. (As noted above, for "mind," we may substitute "self," or "I.")

It is, I think, impossible to pin down Whewell's view of matter. In certain passages "matter" seems to mean simply resistance. In others matter is something which we must posit in order to explain the possibility of resistance. And "resistance" itself has two quite different meanings for him. Sometimes it means something which I perceive. It is a "formed sensation." At other times it is equated with the scientific term "inertia."[25]

I think one of the most difficult statements in Whewell to elucidate is the statement that the nature of matter is perfectly known to us. My interpretation of this passage is that the nature of matter is perfectly known to us because, (a) matter is known as a cause and, (b) it is, therefore either a Conception or an Idea, and (c) in the case of this particular Conception (or Idea) "we"--*i.e.*, the physicists, and especially Newton--have exhaustively stated the *fundamental* axioms pertaining to it. But while we may know all these things about matter, Whewell is generally vague as to what matter "really is," *i.e.*, he is vague concerning its ontological, or substantial character. He does not, for example, say whether or not matter could exist if nothing else existed.

In addition to God and mind and, possibly, matter, there are a number of entities which have some of the characteristics, and which perform some of the functions, traditionally ascribed to substances. Whewell calls these entities media.[26]

whether they be said to have that kind of reality supposedly possessed by substances--*i.e.* "ontological" reality--is, in the first place, probably more a matter of taste than anything else. And, in the second place, is probably of no great significance. But the epistemological significance of the media as media is very great.

There are two kinds of media, subjects and objects. And they perform a function which, from the standpoint of epistemology, is of the greatest importance. Knowledge, for Whewell, can best be defined as the relating of particulars--*i.e.*, of things which can be perceived as discrete--by means of media. Subjectively- *i.e.*, from the standpoint of the subject become self-conscious--particulars are related by means of the activities of the subject. Objectively, they are related in the object. From the standpoint of the object--*i.e.*, taking the object in and of itself--the object unites, among other things, Ideas and sensations. Also the laws and causes of phenomena, found out by induction, are said to "apply to" objects. The object, then, is a medium in which the laws and causes are exhibited. The object, for example, comes to be known as "law abiding." Or it comes to be known as the effect of this or that cause.

Media, then, are among the most important grounds of the possibility of knowledge. We define knowledge as the relating of particulars. And we ask, How is this possible? The answer to this question is, in part, Because there are media.

Another "entity" which often seems to function as a medium,

is knowledge itself. Though subjects and objects may accomplish the relating of particulars, one might still ask, How are subjects and objects related? And Whewell's answer, sometimes, is that these are related within knowledge, especially within that knowledge which he calls the philosophy of science.

8. Whewell's Realism and His Employment of the Correspondence Theory of Truth

There can be no doubt that Whewell is an epistemological realist. In his case, realism involves at least the following commitments. (a) Objects and subjects are real in that their existence is known through immediate perception. Or, to borrow a phrase from Kant, their existence is known "upon the immediate witness of . . . self-consciousness."[27] There is, in other words, no need to resort to inference to establish the reality either of myself or of "outer" objects. (b) Abstract or general terms include among their denotata Ideas and Conceptions. Ideas and Conceptions are as real as subjects and objects. And their reality is capable of being known intuitively.

However, realism, as I understand it, generally allows for the possibility of complete knowledge. But there is, in Whewell, an element of scepticism derivative from two sources. (a) If Ideas are held to be the Ideas of a divine Mind, then we cannot hope to know them in the way in which the divine Mind knows them. (b) If there is, as Whewell sometimes asserts, an Absolute, we cannot know it in and of itself. We can only know that there is an Absolute. But the statements which *we* call "true" will, from the standpoint of the Abso-

lute, be only "partly" true.

The above grounds for scepticism notwithstanding, we can still employ a correspondence theory of truth. A statement will be said to be true when things are given the same relation in the statement that they have in reality. It is necessary to add the proviso that the "things" so related may frequently themselves be true statements.

This is not a completely satisfactory account of what Whewell means by "true." He does give essentially the above definition--"A statement is true when things are given the same relation in the statement that they have in reality"--except that where I have used "things" he uses "facts." I have resorted to "things" here for two reasons. (a) I have not, as yet, introduced Whewell's term "fact." "Fact" is a term which he uses to denote anything which is known to be real, or any statement which is known to be true. (b) Since "fact" can denote "that which is known to be true," it does not seem proper to include it in a definition of "true." For to do so means to employ in the definition the term being defined. However, while the above account is not completely satisfactory, it will do for the moment. I have a fuller discussion of what Whewell means by "true" in Chapters III and IV below.

9. The Methods of Knowing

There are three methods of knowing, for Whewell--intuition, induction, and deduction. A major portion of my thesis

is devoted to these methods. I am particularly interested in the grounds of the possibility of employing these methods to arrive at true statements. It is my opinion that Whewell accepts uncritically a correspondence theory of truth and also accepts the view that intuition, induction, and deduction, are all capable of yielding statements which are true in accordance with the definition of "true" offered in the preceding section. My view is that he begins with "true" statements and then works his way "downwards" in order to discover what makes it possible for the above methods to yield true statements.

In very general terms it is Whewell's realistic assumptions which are the grounds of this possibility. These assumptions are broadly that there exist minds (1) which possess numerous dispositional powers and (2) which are the loci of the Ideas and Conceptions that give to experience its formal qualities; and that there exist objects which are perceived as existing in space and time. Objects are also perceived as having numerical properties--i.e., every "thing" has the property of being "one." Objects are perceived as existing independently of the perceiver. And since each object is perceived as "one," objects are, to that extent, perceived as existing independently of each other. Given the above assumptions it is then possible to show that the mind can know some things intuitively, and others by relating them, either via deduction or induction.

10. <u>The Philosophy of Science</u>

The philosophy of science has two major functions, to describe science, and to justify it.

For the most part the description of science can be left to the scientist. But the philosopher can make some contributions here. He can, for example, prepare a history of science. But this must be a "philosophical" history, *i.e.*, a history set forth in terms familiar to the philosopher, but not to the historian. For Whewell, the history of the inductive sciences, for example, is a history of the employment of Ideas. Now, Ideas do not occur in chronological order like the events--*e.g.*, the *discovery* of the laws of motion--which the mere historian might record. Ideas are present, implicitly, from the outset. And they are related to science in two ways since science "unfolds" the Ideas, making them more intelligible, and it also relates phenomena in terms of Ideas. A philosophical history of the inductive sciences, then, traces the unfolding, over a period of time, of the Ideas, and the relating of phenomena in terms of Ideas. Another project which is of some interest to Whewell, as a philosopher expounding science, is to classify the sciences.

The second task of the philosophy of science--the justification of science--belongs to philosophy alone. For Whewell, the major challenge is to relate subjects and objects which are given to perception as discrete. This relating is not, as Comte thought, simply a matter of discerning phenomenally given relations among phenomena. It is instead, a matter of

justifying a principle which the scientist merely employs without having a "philosophical" justification for its employment. The principle is that objects (and subjects) which are perceived as discrete <u>can be</u> related. Whewell's position is that the principle is justified when we show that in order to be perceived at all, the objects of perception <u>must be</u> perceived as related. He also holds that the relations which the objects of perception are "discovered" to have, originate in the acts of the knowing subject. Since the mind can be aware of its own acts, science, which is the result of those acts, is shown to be possible. The argument ultimately reduces to a tautology: perceived objects are always known objects. And the same acts of the mind produce both the object as perceived, and the object as known. For the "object as perceived" and "the object as known" are seen, as a result of philosophical inquiry, to be synonymous expressions. And since every act of the mind involves at least one of intuition, induction, or deduction, these three are seen to be the methods of knowing, <u>i.e.</u>, are seen to be the methods which yield true statements.

CHAPTER III

THE DATA OF KNOWLEDGE ACCORDING TO WHEWELL

A. The Data of Intuition

1. The Data of Intuition Distinguished from Intuition and from the Results of Intuition

To state what the data of intuition are, for Whewell, or for that matter, to state what they are for anyone, is made difficult by the fact that it is not easy to distinguish rigorously the intuitive act, the results of intuition, and the data of intuition. At first view there seems to be but a single entity here. Can we, for example, say that we intuit colours? Is it not, rather, the case that colours simply are intuitions? The datum (the greenness, for example), the intuitive act (the intuition of greenness), and the results of the act (the fact that one is intuiting greenness), seem to be simply the same thing seen from three different points of view. I think, however, that while the datum, the intuitive act, and the results of the intuition may be one and the same experientially--or, as Whewell would say, "in fact"--nevertheless we can distinguish them for purposes of analysis and exposition.

2. Intuition Versus an Intuition

The first thing which needs to be done where Whewell's

use of "intuition" is concerned is to distinguish between intuition and <u>an</u> intuition. By intuition is meant a power of the perceiving mind. By an intuition is meant anything which is known intuitively.

Whewell stipulates, in one passage, that intuition is <u>only</u> a power of the mind to discern that a perceived object is both one, and many.[28] He also speaks of a power of the mind which he calls "the sight which produces knowledge":

> As the poet says:
>
> It is the mind that sees: the outward eyes
> Present the object, but the mind descries.
>
> And this is true of the sight which produces knowledge.[29]

"Sight," here, is a power of the mind to perceive <u>immediately</u>. And since objects are perceived <u>immediately</u> as one and many, or else are not so perceived at all, intuition appears to me to be a particular instance of the sight which produces knowledge.

For Whewell there are many kinds of knowledge which are known immediately. And it seems hard to justify singling out one of them--the knowledge that an object is both one and many--and ascribe this knowledge to a particular, named, dispositional power when other similar kinds of knowledge are not ascribed to particular powers but to a "general" power--the sight which produces knowledge. The point which Whewell is making is that there are some things which are either known immediately, or not at all. As examples of such knowledge I shall cite (<u>a</u>) the knowledge that a perceived object

is both one and many; (b) the knowledge that something which I perceive is perceived as an object; (c) the knowledge that some object which I perceive is red in colour; (d) the knowledge that a given conclusion follows from certain premises; (e) the knowledge that the orbit of Mars is an ellipse; and (f) the knowledge that certain axioms[50] exhaustively describe the Idea of cause. In all of these cases knowledge is, according to Whewell, given immediately, or it is not given at all. If we relate these six items to a power of the mind, the same power is appealed to in each case, it seems to me. I think, then, that what Whewell calls "intuition" is but a special instance of "the sight which produces knowledge."

For this reason I cannot see that it is necessary to make the distinction between them which he makes. I, therefore, propose to extend the term "intuition" to include "the sight which produces knowledge." I am of the opinion that this in no way falsifies Whewell's account. And it does simplify it. Instead of two expressions--"intuition" and "the sight which produces knowledge"--we may substitute the single term "intuition." Admittedly, since intuition is an instance of the sight which produces knowledge, and not *vice versa*, it might be better to drop the term "intuition" and retain the expression "the sight which produces knowledge." But I propose to drop the latter and retain the former since "intuition" is a less cumbersome, and a less metaphorical, expression.

I have a further reason for wishing to mean by "intui-

tion" in Whewell's case more than his stipulative definition of the term allows. "Intuition" is the name which many philosophers give to a sight which produces knowledge if we mean by "sight" in this case a power which gives us immediate knowledge. Mill, for example, uses "intuition" in this way:

> Truths are known to us in two ways: some are known directly, and of themselves; some through the medium of other truths. The former are the subject of Intuition.[31]

One of my intentions is to compare the philosophies of Mill and Whewell. A very important item which they have in common is their agreement that, as Mill says above, some truths are known directly. Since Mill uses, in this context, the term "intuition" one can speak of intuitive truths in his case. But, in Whewell's case, if one is to employ the terminology which he provides, one has occasionally to speak at one and the same time of "intuitive truths" and of truths which are the result of the sight which produces knowledge. Since "the sight which produces knowledge" in Whewell is equivalent to "intuition" in Mill <u>to the extent that</u> each expression designates our ability to know some things directly, I propose to substitute Mill's term for Whewell's. The major advantages to be gained by this are mechanical. But they are, nevertheless, considerable. For example, as a result of this substitution, I can set up such parallel sections as "The Data of Intuition According to Mill," and "The Data of Intuition According to Whewell." Without the substitution of terms suggested above, I should have to compare the data of intuition,

in Mill's case, with the data of intuition and the data of the sight which produces knowledge, in Whewell's case. This could be done. But it would be awkward. And since there is no misrepresentation involved in substituting "intuition" for Whewell's two expressions "intuition" and "the sight which produces knowledge" I propose to do this. The only difficulty, theoretically, which might arise would originate in the fact that what Whewell means by "intuition" as he has stipulatively defined it, becomes an instance of, but is not equivalent to, since it is less extensive than, what I shall mean by "intuition" in his case. But I think that in practise, no difficulties will arise from employing "intuition" in the manner in which I propose to employ it.

By "an intuition" is meant anything which is known intuitively. Intuitions differ from other items of knowledge in that intuitions stand in an immediate relation to the subject, whereas other items stand in a mediate relation to the subject. For example, that my desk has spatial properties is an intuition for me. The definitive character of an intuition is the impossibility of denying its "truth." By this is meant that one can know at once that certain statements are true. For example, I know intuitively that my desk has spatial properties. In this way it is possible to distinguish intuitions from truths which have to be established either inductively or deductively. There may be some virtue in distinguishing intuitions from intuitive truths. But if there is, Whewell does not make the distinction.

3. The Data of Intuition

Whewell's analysis does not lend itself very easily to the project I have in mind at the moment, namely, to discover the data of intuition. He does not distinguish sharply enough among that which is intuited, the intuitive act, and the results of the intuitive act. By the data of intuition I intend to signify only that which is intuited, thereby separating it from the activity of intuiting it, and from the results of intuiting it. Nevertheless, one can meaningfully say that, for Whewell, all possible intuitions may be said to be the data of intuition. They are data in three senses at least: (a) They are *qua* truths, objective to the observer. The observer cannot create them. Instead they are "there" to be known. (b) They are "presented to" a power, which I have called "intuition" and which Whewell calls "the sight which produces knowledge." And (c) they are amenable to induction.

4. Intuitions Always Cognitive

For Whewell, intuitions are always cognitive. And if we say that intuitions are the data of intuition, it is necessary to stress that the data of intuition must always have the quality of being able to be present to cognition. The quality essential to this end, then, is that true statements known immediately to be true, can be asserted of intuitions. My perception of the desk, for example, is an intuition, because I can assert true statements, known immediately to be true, about the desk. For example, I can say that the desk

has spatial properties. Similarly, the axiom "If equals be added to equals the results are equal," can be an intuition. For I can make a true statement about this axiom, a statement which I know immediately to be true. I can say, for example, "The axiom, 'If equals be added to equals the results are equal' is true."

In order to get at a further property possessed by some intuitions, it is necessary to distinguish between the form and the content of the intuition. All we can assert of the content is that it is perceived as actual. "The desk is brown," is a true statement about an intuitively perceived object. But it relates only to the content of the intuition. All it asserts is that the desk is undeniably perceived as brown. "The desk has spatial properties" is a true statement about the same intuition. It relates to the form of the intuition. But where, in the case of the brownness, all I could assert was that it was undeniably perceived as actual, in the case of the spatial properties, I can assert that they are perceived as universal and necessary.

Since science is more interested in the universal and necessary, than in the merely actual, it is more interested in the form of intuitions than in their content. The scientist does not ignore the content. In fact, it is, in large measure, the content which he hopes to know. But he hopes to know it as universal and necessary, whereas in the form in which it comes to him, it is merely particular and contingent. By simply dealing with the content itself, he cannot eliminate

its particularity and contingency. Experience may, at any time, contradict any merely empirical law. But "we" can establish, at the level of metatheory, that the content of experience always exhibits some form. We can also establish, at the same level, that the forms are universal and necessary. If they were not, experience would not be experience but would be a meaningless succession of conscious states. We can also establish, at the level of metatheory, that the perceiving subject is the origin of these forms. They are not, therefore, merely particular and contingent experiences. They are instead, the very conditions of experience. To present these conditions to intuition is one of the primary goals of knowledge-seeking activities. It is Whewell's belief that these conditions are possible intuitions and, therefore, data of intuition. But to actualize them as intuitions may take a very long time. This is what explains a number of passages in Whewell which are, at first sight, most curious, as when he speaks of "progressive intuitions." He says, for example, that it took the human race centuries to achieve the intuitions which Newton set forth as the laws of motion. And then it was, at first, only Newton who succeeded in perceiving these truths intuitively. But, "theoretically," anyone with sufficient intelligence and training can have the same intuitions that Newton had.

The data of intuition, for Whewell, then, are all possible intuitions. The simplest intuitions, which almost anyone may have, are those possessing the forms of space or time or

number. But some intuitions are very complex. And others are very rare.

Finally, there are, for Whewell, no non-cognitive intuitions. In the early pages of his <u>Philosophy of the Inductive Sciences</u> he asserts that the views set forth in this work are in opposition to the "ultra-Lockian school." Whewell means by this, primarily, that he is rejecting the view which holds that the primary data are non-cognitive sensations, which subsequently become transformed into cognitive perceptions. Perceptions, he assures us, are not transformed, but informed, sensations. That is to say, perceptions are sensations which have been given form. We do not perceive sensations. Sensations are known only to philosophers and physiologists. Instead, we perceive perceptions. Perceptions are intuitions and are, therefore, part of the data of intuition. And while, temporally speaking, perceptions may be our earliest intuitions, they do not, for this reason, have any special significance for knowledge. From the standpoint of knowledge, on its formal side, the most important intuitions are those which can function as axioms, definitions, rules, laws, and causes. And these are generally the last arrived at, temporally speaking.

B. <u>The Data of Induction</u>

1. <u>Two Kinds of Data for Induction</u>

The data of knowledge for Whewell must always be twofold, because of the fundamental antithesis which he discovers in

every knowledge situation. For example, where our knowledge of space is concerned, both the Idea of space, and the Conceptions to which it gives rise, as well as our empirical observations of objects possessing spatial properties, would qualify as relevant data.

2. Facts

The term which Whewell uses most frequently to name the data of induction is the term "facts." "Facts" can denote the objects of perception, and it can denote true statements about objects. A tree is a fact. And the statement, "Oaks are trees" is a fact. But concerning the status of Ideas and Conceptions as facts, Whewell's position is not clear. Certainly any true statements about Ideas and Conceptions are facts. And he sometimes speaks as if _any_ statement which "really is" true, is a fact, whether it be _known_ that it is true or not. But he cannot hold this view and at the same time maintain his distinction between Fact and Theory. One characteristic of a theory is that it may be true. And some theories are true. Now what distinguishes _theories_ which are true from _facts_ is that true _theories_ are not known to be true in the same manner in which facts are known to be facts. Facts are known to be facts immediately, whereas theories, even if true, have to be proven. Once their truth becomes "self-evident" they cease to be theories and become facts. Since knowledge is constantly changing or progressing we cannot state absolutely what part of knowledge is theory and

what part is fact. What is theory at one time is fact at another.

Having noted, then, that theories may subsequently become facts, thereby becoming amenable to successful induction, we may say that for Whewell induction consists in the colligation of facts, and that facts are the data of such colligation.[32]

There is for Whewell the philosopher, as opposed to Whewell the scientist, nothing which is merely a datum which precedes some theory for which it is a datum. At least, this is his usual approach. Occasionally he does refer to a given, unorganized, sense manifold which, temporally speaking, comes first. For example, in the following passage, the word "before" as it is used in the second sentence, suggests that sensations occur, then an act of the mind unites the sensations, and finally we perceive the object.

> We have thus before us one tree; but this unity is given by the mind itself. We see the green and the brown, but we must _make_ the tree before we can see _it_.[33]

But usually it is impossible to discover which comes first, temporally speaking, the object or the sensible representations of it. And the most accurate interpretation of Whewell seems to be to say that regardless of which comes first temporally speaking, that which comes first, as far as theory of knowledge is concerned, is the object as it is known, or perceived. Whewell gives a more adequate account of the form of knowledge than of its content, except to the extent that he considers that the forms of perception determine the content of perception.

3. Acts of Intuition and the Data of Induction

Whewell makes it reasonably clear that what is given in perception is, in large measure, determined by acts of judgment on the part of the interpreter: "Man is the interpreter of nature, not the spectator merely."[34] Now, if what is given in perception is determined by the acts of judgment of the subject, is it correct to state, as I have done above, that our knowledge properly begins with the object? Does it not, rather, begin with acts of the subject? My answer here is that whatever may be the nature of the priority which exists between the object and the acts of judgment, it is not expressed by Whewell as a temporal priority. Whewell's method is not some variant of Locke's "historical, plain, method." It is undeniable that Whewell recognizes that temporal considerations must enter into our account of knowledge. In fact, he refers to both of his major works--the History of the Inductive Sciences and the Philosophy of the Inductive Sciences--as being historical in their approach.[35] But to get at the act of judgment which constitutes a colligation he seems to regard as impossible, and refers to it as a mystery.

> The process of induction includes a mysterious step, by which we pass from particulars to generals, of which step the reason always seems to be inadequately rendered by any words which we can use; and this step to most minds is not demonstrative, as to few is it given to perform it on a great scale.[36]

I cannot, for example, recall that inference which is the ground of my perception of distance.[37] And, since I cannot

recall it, it cannot be datum. Nor can I use as a datum my ability to recognize kinds of things, even though I may be well aware that "we must think of things as of different Kinds, in order that we may know anything about natural objects."[78] The kinds will be data, but the acts which yield them to my consciousness will not.

What kinds of things there in fact are, we discover through empirical observation. But that which links together members of one kind within our knowledge is an idea supplied by ourselves, not a datum apart from the objects as linked together. For example, where those acts of judgment which yield the Newtonian theories are concerned, I, perhaps, have never performed the acts at all. And the acts of judgment which supplied these concepts in Newton's case defy analysis. It would, therefore, be absurd to take such acts as the data of physics. Instead we take objects as our data.

4. The Inconstancy of the Data

Whewell's major interest in objects is not, What objects, or kinds of objects, are there? but rather, Why are objects as they are? And his answer is, basically, that objects are as they are because, in the first place, they contain an irrational element--sensible intuitions. And, in the second place, objects are as they are because, in order to be, they must be thought:

> This Antithesis, as I have . . . remarked, is stated in various ways . . . . I have further remarked that the elements thus spoken of, though opposed, are inseparable. We cannot have the one without the other. We

> cannot have thoughts without thinking of Things; we cannot have things before us without thinking of them.[39]

As our perspective shifts during our discussion of knowledge, so the objects we are dealing with may undergo change. In one context the data will be objects like Mars and the sun. In another the data will consist of theories about these bodies.[40]

It is also necessary to remember that the perceiver is never merely passive. That the perceiver is to some extent passive, Whewell acknowledges; and it is possible that in this sense something common is given to all observers. For example, according to the science of physiology when I look at a tree, an inverted image of the tree appears upon my retina. But this inverted image does not appear within my consciousness. It does not, therefore, constitute a constant, or common, datum to which all observers might refer. I am conscious by means of it, but am not conscious of it. I see a tree 100 feet high, not an inverted image of a tree a fraction of an inch high:

> An inverted image of the objects which are looked at is formed upon the retina. . . . But we cannot with propriety say that _we_ perceive, or that our mind perceives, this image; for we are not conscious of it, and none but anatomists are aware of its existence.[41]

In this sense, then, all the data empirically given to perception have to be presumed to be constructed, not literally "given." The perceived object is an inference from sensation. And since the inferences are drawn by "interpreters of nature" who range from simple savages to Newtons, they will

vary accordingly.

5. Sensations

> We scarcely ever dwell upon our sensations; our thoughts are employed upon objects; we regard the impressions upon our nerves, not for what they _are_, but for what they _tell_ us . . . . Sensations are the rude Matter of our perceptions; and are nothing except so far as they have _Form_ given them by Ideas.[42]

One point which Whewell is emphasizing here is a mistake which he feels has been made by those British empiricists who held ideas to be simply the continuing effect of more primitive perceptual experiences, sensations. Whewell's position is that sensations _per se_ do not enter into consciousness and, therefore, are not perceptual experiences at all. Sensations do not have some proper form of their own which is converted into some other form. Sensations themselves have no form. Instead they are the "matter" of perception and are given form by some act of the mind. Sensations are something which we arrive at by analysis; they are not given directly in experience. And they are of interest to the philosopher, but not to the inductive scientist (unless he happens to be a physiologist.) For the scientist induction begins with perceptions of objects. He is not interested in, and has no need for, the philosophical entity called a sensation. The sciences of optics, acoustics, and thermotics, for example, are independent of any anatomical analysis of the corporeal conditions of vision, or hearing, or feeling warm.[43]

6. The Cognitive Quality of the Data of Induction

The term "data," for many people, has the connotation of

that which precedes knowledge. But this is not Whewell's position. At whatever level one might attempt induction, one would have a knowledge of the data. That is to say, the data would always be meaningful elements within a conceptual scheme. Even primitive man, we must hold, did not come upon "pure data," "raw data," "brute facts." To be known is to be related. And for Whewell there is nothing which is given to consciousness as absolutely unrelated. Possibly to primitive man Mars may simply have been "this blob." But, even so, it would be given as "this blob" and as not "that blob." It would be given in space and time. It would stand in certain relations to the observer. And so on. To separate Mars from what is known about it is impossible, both to the primitive savage and to the most sophisticated astronomer. One's knowledge of Mars may change. But if it is possible to take Mars as a datum, then one knows what it is that the datum is <u>for him</u>. It is correct to say that data are what precede induction. But it is not correct to say that data are what precede knowledge.

Although the objects of sense--<u>i.e</u>, the facts of experience--contain an irrational element, they also contain a rational element. And the colligation of facts requires, primarily, "hitting upon" the relevant Idea in terms of which to colligate them. For example, even though scientists were not having much success in colligating living things and the events peculiar to them, Whewell could be optimistic about the eventual success of such endeavours. For he had established, at least to his own satisfaction, that there must be

an Idea of life in terms of which biological events could be colligated. It is true that no scientist has succeeded in clearly formulating the Idea. But all that is required is human ingenuity, patience, and genius, human qualities in which Whewell had almost unlimited confidence. Newtons are rare. But the human race continues to produce them. And sooner or later some "Newton" will do for biology what the historical Newton did for mechanics. Certain events and objects have certain characteristics by virtue of which we refer to them as biological. Their "biological" character is not the result of our sensations, the irrational element. Instead it is the result of the activities of the knowing mind, the rational element which unites the sensations. And, therefore, it is in principle possible to make the Idea, which is the ultimate ground of their form, explicit, and to explicate the Idea into conceptions which will colligate that which is "given" as having the qualities peculiar to life. Our lack of success to date should not discourage us. It took mankind thousands of years to colligate the facts relevant to the movements of the planets. And even Kepler himself failed dismally nineteen times. Yet he succeeded gloriously on the twentieth.

But even the philosopher recognizes that some facts will never be completely "Idealized." Since the fundamental antithesis is a necessary antithesis there will always be facts which are perceived merely as facts. In the Absolute the antithesis may be overcome. But absolute knowledge is beyond

human grasp.

7. Potential Data Versus Actual Data

While all inductions employ facts as data, it does not follow that all facts will prove amenable to induction. One's opinion here is determined by whether one looks at facts from the scientific point of view or from the philosophical point of view. From the standpoint of the scientist, that certain facts are data suitable for induction is only established after the induction has been performed. It is easy to show, by an examination of the history of the inductive sciences, which facts have lent themselves to induction. Such facts, of course, merit the term "data of induction." But the only proof which can be offered, on the scientific level, that certain other data are equally amenable to the same method is to colligate those facts by means of induction. And such a proof frequently cannot be supplied.

From the standpoint of the philosopher all facts are "in principle" amenable to induction, to the extent that they contain an ideal element. For example, to the extent that facts are spatial in their form they "should" prove amenable to induction under some spatial Conception.

8. Ideas and the Data of Induction

It is difficult to be sure whether or not the data of induction actually possess, as attributes or qualities, the Ideas and Conceptions which bring about their colligation by means of the inductive process. In other words, are number,

likeness, irritability, and the rest, among the data? I do not think that Whewell can give a direct "yes" or "no" answer to this question. His attitude is that as a result of induction the original data subsequently are known as possessing a quality which they acquire by means of the induction but which they were not known to possess prior to the induction. And to the extent that "data" refers to that which precedes the act of induction, it is necessary to hold, where any given induction is concerned, that the data do not possess as an attribute the Conception which accomplishes their colligation. But for subsequent colligations, or inductions, they would possess this attribute. It is for this reason that what is a fact for one person is not a fact for another. Prior to the work of Kepler, the planet Mars as a datum, did not possess the attribute of moving in an elliptical orbit. But for subsequent workers in the field of astronomy Mars' elliptical orbit became a datum.

In most general terms, the data of induction, for Whewell, are facts. And, as indicated above, facts have a chameleon-like nature. Induction is conceived as the colligation of facts. And the facts which are colligated may be various, ranging from the most particular of observations to laws of a high degree of generality. It is a commonplace with Whewell that as an empirical science develops, its data become increasingly ideal:

> I trust . . . that we have yet many new laws of nature still to discover; and that our race is destined to ob-

> tain a sight of wider truths than any we yet discern, including, as cases, the general laws we now know, and obtained from these known laws as they must be, by induction.[44]

3. Two Criticisms of Whewell's Account of Facts as the Data of Induction

The first criticism I have to offer of Whewell's account of facts as the data of induction is that his term "fact" is not rigorously enough defined. It remains too largely a common sense term and lacks the precision which it requires in a philosophical system in which it plays such an important role.

The second criticism I have to offer is that it is very difficult to know whether there is a common world of fact. I think Whewell handles very well the matter of showing that there is a common world which, in principle, all can come to know. This common world is the world of the scientist. But even in relation to the scientific world there is a problem. If one's theory determines one's data, in the sense that for astronomers who accepted the Keplerian laws, the elliptical orbit of Mars became a datum, the question I should like to raise is, Does a true theory determine the data truly? Whewell's position here is that a new theory does not falsify an old one. Instead, an older one is "taken up" into the new theory. But what, precisely, it means to be "taken up" in this way, Whewell does not succeed in stating. Nor does he say what happened to the data which the old theory established, and which the new theory destroys. On Whewell's arguments it would have to be allowed that the Ptolemaic theory

established the fact that the sun goes around the earth. And I should like to know what happened to this "fact" when the theory was abandoned. Whewell seems to me to be in a particularly tight corner here because of his subscription to a correspondence theory of truth. The way he gets out of this corner is in terms of some vaguely glimpsed dialectic in which old theories neither die nor slowly fade away, but are, instead, "taken up" into a new theory.

C. The Data of Deduction

The expression "data of deduction" has an odd sound about it. But I find that at least one reputable author sanctions this usage. J.K. Feibleman offers as one definition of "datum" the following: "In logic: facts from which inferences may be drawn."[45]

Where Whewell is concerned, the data of deduction or, if one prefers, the premises of deduction, frequently originate with induction:

> The doctrine which is the hypothesis of the deductive reasoning, is the <u>inference</u> of the inductive process. The special facts which are the basis of the inductive inference, are the conclusion of the train of deduction. And in this manner the deduction establishes the induction. The principle which we gather from the facts is true, because the facts can be derived from it by rigorous demonstration. Induction moves upwards, and deduction downwards, on the same stair.[46]

Induction consciously employed yields primarily definitions, laws, and causes. In inductive reasoning one proceeds towards these. But deductive reasoning begins with these and asserts them as axiomatic.[47] What makes deduction possible

is that, as a result of the inductive step, the particular case is included in a general proposition. Without the general proposition, it would be impossible to deduce the particular one:

> The Mathematician asserts the Laws of Motion, seeing clearly that they (or laws equivalent to them) afford the only means of clearly expressing and deducing the actual facts.[48]

If, then, deduction can properly be said to employ data, the data are laws, definitions, and axioms. All three of these I discuss in detail elsewhere, so there is no need to discuss them here.

In the end it will be discovered that there is no hard and fast distinction between the data of induction and the data of deduction. But the laws, definitions, and axioms which form the data of deduction do have the peculiarity that they are mental facts, not physical ones. As known facts, they are ideal, not sensible. The orbit of Mars, expressed as a law of the motions of Mars, is a datum suitable for deduction. The ellipse itself, once it has been established that Mars moves in an ellipse, is a datum for induction. It is the true proposition, stating precisely the character of the ellipse, which becomes a datum for deduction.[49]

D. The Data of Knowledge and Their Relation to the Totality of What Is

For Whewell, the data, intuitive, inductive, and deductive, available to us, do not exhaust the totality of what is. In the first place, knowledge may be described as the Ideali-

zation of facts, and Whewell tells us this process can never be completely carried out. There will always be, therefore, facts which are in principle, data,but which have not actually been so employed. In the second place, in addition to the facts available to us, there exist the principles by virtue of which the universe is constituted and maintained. These may exist as Ideas in some Divine Mind, and to such a mind they would be perfectly known. But to us they are not, and will never be, fully known. "The intellect of man cultivated by science . . . discloses to him some things in some measure."[50] But this disclosure is never complete. "[Man's] knowledge, his science, his Ideas, extend only as far as he can keep his footing in the shallow waters which lie on the shore of the vast ocean of unfathomable truth."[51]

CHAPTER IV

THE METHODS OF KNOWING ACCORDING TO WHEWELL

Introduction

Before talking about "The Methods of Knowing According to Whewell" it is first necessary to point out a certain ambiguity in Whewell's exposition. The ambiguity arises because he holds that there is only one method of knowing phenomena, and yet that there are three methods of knowing phenomena. There is only one method of knowing in the sense that the scientific method is the method. There are three methods in the sense that the scientific method is an amalgam of three methods of knowing--induction, deduction, and intuition. I shall treat the three methods individually, but shall also try to show how they function as components of a single method, the scientific method.[52]

I should also like to suggest that, for Whewell, in addition to the scientific method there might also be a philosophical method. It is a reasonable speculation to suggest that since science, which gives us knowledge of experience and nature, has its method, the philosophy of science, which gives us knowledge of science, may have its own method. The method of science is to some extent determined by its subject matter. It would not, therefore, be foreign to Whewell's orientation

to knowledge to say that since the subject matter of the philosophy of science is to some extent different from the subject matter of science, the method of the philosophy of science might, to that extent, differ from the method of science. If it is the case that science and Whewell's philosophy of science have different methods, Whewell says nothing about this difference. If there is a difference between the method he employs and the method he advocates he does not seem aware of it. But I am not at all convinced that in his Philosophy of the Inductive Sciences he is employing the scientific method as he conceives it to be. He would hold, as nearly as I can make out from the few passing remarks he makes on the subject, that the method of the philosophy of science, and the method of science, are alike in that each employs induction, deduction, and intuition. I do not deny that this is so. But it is clear that the fact that each employs induction, deduction, and intuition does not establish that the method is necessarily the same in each case.

Whewell sometimes suggests that the methods of science and the philosophy of science are the same for the reason that the distinction between science and the philosophy of science is an arbitrary one. The philosophy of science is not precisely a part of science. Nor is it a science among sciences. Instead it is the fruition of science. "Science" might be defined as an intellectual "grasp" of experience. But science is itself a part of experience. Our intellectual grasp of experience, then, is not complete until it grasps all of ex-

perience, including scientific "experience." In providing this all-inclusive grasp, the philosophy of science renders science--*i.e.*, the intellectual grasp of experience--complete. One way in which the philosophy of science does this is to make clear what the scientific method is, and what it accomplishes. In making clear what the scientific method is, and what it accomplishes, the philosophers of science employ induction, deduction, and intuition. Apart from these there are no methods to employ. This is not to say that there are no other sources of knowledge. For there is at least one other source, for Whewell, revelation. But revelation can hardly be called a method. Furthermore, whatever is revealed must be comprehended by us inductively, deductively, or intuitively. There are, therefore, for Whewell, three basic, non-reducible, methods of knowing--induction, deduction, and intuition--which I shall proceed to discuss in order.

A. The Method of Induction

1. What Induction Is Not

Before I attempt to state what induction is, for Whewell, I should first like to make clear what, according to him, it is not.

(*a*) Induction is not mere enumeration. It may be preceded by enumeration, but it is other than, and more than, enumeration. (*b*) Induction is not description. Induction includes a new conception which goes beyond description. Kepler, in asserting the orbits of the planets to be ellip-

tical, did something other than <u>describe</u> what he had observed. (<u>c</u>) Induction is not a practical skill. The rope dancer, as a result of experience and practice, is able to keep his balance. And it is quite true that what he has been encountering in his attempts to keep his balance, is the force of gravity, and especially that entity known as the centre of gravity. But the skill of the rope dancer is not the result of induction:

> The rope dancer does not dance by Induction, any more than the dancing dog does. To apply the terms Science and Induction to such cases, carries us into the regions of metaphor . . . . Induction for us is general propositions, contemplated as such, derived from particulars.[53]

(<u>d</u>) Induction is not a method for arriving at statements of the same order of particularity as that possessed by the statements which it employs as data (or which it regards as "facts," to use Whewell's term). Instead, as the foregoing quotation asserts, induction is present only when the subsequent statements are of a higher order of generality than are the original propositions. (<u>e</u>) Finally, and this seems a rather extreme way in which to state the case, induction is not a form of reasoning. This is Whewell's way of indicating his agreement with Aristotle that all belief arises <u>either</u> from syllogism <u>or</u> from induction, and with Bacon that there are two ways of knowing, by argument, and by experiment. It must be pointed out, however, that although Whewell makes the above distinction between induction and strict, deductive, reasoning he is nevertheless careless in maintaining this dis-

tinction as far as his terminology is concerned, and frequently speaks of "inductive reasoning" as, for example, in the following passage:

> It has been said that inductive and deductive reasoning are contrary in their scheme . . . . And this is truly said; but though contrary in their motions, the two are the operations of the same mind travelling over the same ground. Deduction is a necessary part of induction. Deduction justifies by calculation what Induction had happily guessed.[54]

In deduction, every step of the argument can be set forth syllogistically, so that anyone can understand how to get from one point to another. But in induction, there is always a gap, a "leap" which the mind makes, and which is not subject to rules. As a result, induction to a considerable extent defies rules and, largely for this reason, it is impossible to set down an infallible method of discovery. In order to talk about induction it is necessary to include such terms as "guess," "hypothesis," "theory." "Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin;--some fortunate cast of intellect rising above all rules."[55]

2. What Induction Is

(a) Induction Distinguished from Science

Whewell distinguishes the factual from the purely formal sciences. One of the grounds for this distinction is that the factual sciences employ induction, the purely formal sciences do not. There are two ambiguities in Whewell's account of this matter. And although I have tried to avoid these in my

exposition, I have not been entirely successful.

The first ambiguity is that, having made a distinction between the factual sciences and the method of induction, he frequently equates them. He says, for example, "Induction is experience or observation consciously looked at in a general form."[56] But this would serve equally well to define what he means by "science." The second ambiguity is that having made a distinction between the factual and the purely formal sciences, he proceeds to show that the factual sciences are highly formal. In fact, if we consider Whewell's *Mechanical Euclid* a success, it turns out that the reputedly factual science of mechanics can be set up as a purely formal science. It remains factual only in that it has a reference to objects of possible experience. But so do the "purely" formal sciences. If the *Mechanical Euclid* is a success, then mechanics as a science can be derived solely from Ideas and Conceptions, without the employment of induction at all.

I have tried to overcome the above ambiguities in the following manner. First, I have maintained as rigid a distinction as possible between the factual sciences and the purely formal sciences. Secondly, I have maintained as rigid a distinction as possible between the factual sciences and the method of induction.

In the present section, which I have entitled, "What Induction Is," my chief interest is in induction as a method. But since Whewell frequently defines induction in terms of

its results, it does not seem possible to discuss the method without discussing its results. It is only when we appreciate its results that we understand what the method of induction is. It is that method which, employing such and such data, accomplishes such and such results. In order to comprehend induction we have to "see it in action." We have to follow through the scientific activity from the selection of "true facts" to the establishment of laws and causes of phenomena. From the standpoint of the scientist, induction is an intellectual activity which he performs, an activity which is induction-in-act. And this activity we can define as "the colligation of facts." The philosopher of science wants to know, Is there any method to this activity? Can it be described in such a way (1) that we can understand what goes on in the scientist's mind, and (2) that it can be deliberately employed by other people? Whewell's answer is that it can be so described.

In the following section I subdivide the scientific process into six items. By discussing these six items, I hope to make clear what Whewell's view of induction is. But before I attempt to locate induction as a "step" in the scientific process, I wish to make three very general, and, admittedly overlapping, statements about induction. All three are statements which can easily be supported by quotations from Whewell. (1) Induction is a way of searching for laws and causes of phenomena. Not only is it a way of searching, it is the way which is most likely to meet with success. (2)

Induction is a way of establishing some of the objects which scientific terms denote and the true statements, of a high order of generality, about the objects which scientific terms denote. (3) Induction is the method which relates facts by means of Ideas and Conceptions.

(b) The Six Processes which Constitute the Scientific Activity in which Induction Is Employed

In attempting to subdivide the scientific activity into "parts" one is really attempting to separate the inseparable. No such attempt, then, can be entirely successful. Nevertheless, in order to talk about scientific knowledge, Whewell finds it helpful to speak of it as the result of a series of processes.

The series is made up of six items: (i) the decomposition of facts; (ii) the measurement of phenomena; (iii) the explication of Conceptions; (iv) the induction of laws of phenomena; (v) the induction of causes; and (vi) the application of inductive discoveries. I shall deal with all six of these, but I am chiefly interested in items (iii), (iv), and (v).

(i) The Decomposition of Facts

What Whewell discusses under the headings the "decomposition of facts," and the "measurement of phenomena" could be discussed under the single heading of "observation." Nevertheless, there are two distinguishable problems involved here. So I shall accept his terminology, and will begin with the decomposition of facts.

Where the decomposition of facts is concerned, the first problem is to "see" what is "really there":

> When we enquire what Facts are to be made the material of Science, perhaps the answer which we should most commonly receive would be, that they must be True Facts, as distinguished from any mere inference or opinions of our own.[57]

The problem then is to determine under what conditions facts can take on the characteristic of becoming "True Facts."

When we attempt to discover these conditions we find that the "answer" implied in the above quotation is of no help. The above quotation suggests that in seeking for facts we must accept as facts only what is "given," and set aside, as non-factual, whatever we as perceivers may have contributed to the perceived object. But to accomplish this separation is impossible:

> We cannot obtain a sure basis for Facts, by rejecting all inferences and judgments of our own, for such inferences and judgments form an unavoidable element in all Facts. We cannot exclude our Ideas from our Perceptions, for our Perceptions involve our Ideas.[58]

Since we cannot reject as non-factual the Ideas which all facts involve, Whewell suggests that we begin by attempting to discern the Ideas which the facts include. In conformity with this method of procedure, we may formulate three rules of procedure.

First, if facts are to be used as the material of science, they must be such that they can be "<u>referred to Conceptions of the Intellect only</u>, all emotions . . . being rejected or subdued."[59] Employing this rule we can reject, as being of

no interest to science, all "fearful," "marvellous," "portentious," "prodigious," and the like, observations. Science will not accept as facts tales of armies warring in the sky, tales of fiery dragons, and so on. Such "phenomena" are generally "observed" when the observer is in a highly emotional state. And "we cannot make the poets our observers."[60]

Secondly, the facts are to be observed, as far as is possible, with reference to the most universal, exact, and simple of our Ideas. Of these, space, time, and number are the most universal, exact, and simple. The Conceptions of space, time, and number, of course, cannot always be employed. An example of a case in which they could not be employed would be when our data are laws. But when these Conceptions can be employed, they should be.

Thirdly, even when the Conceptions derivative from space, time, and number can be employed, we cannot always stop with them. For example, in order for the science of mechanics to arise the facts had to be observed as referring to four fundamental Ideas--Space, Time, Force, and Cause.

By the employment of the above rules the observer resolves the complex appearances which nature offers into limited, definite, and clearly understood portions. "This process we may term the Decomposition of Facts. It is the beginning of exact knowledge--the first step in the formation of all Science."[61]

The employment of the above rules will not necessarily

result in scientific knowledge. But there can be no scientific knowledge without their employment.

(ii) <u>The Measurement of Phenomena</u>

The measurement of "facts" serves two main functions. One of the functions of measurement is to provide facts which are definite and objective--*i.e.*, public. The second function of measurement is to provide facts which may be amenable to certain mathematical operations. Our knowledge of the history of science assures us that these methods have often proven useful in our attempts to understand nature.

Where the measures themselves are concerned, some are natural, some are artificial. Examples of natural measures are number and the sidereal day. Examples of artificial measures are the foot rule and the pound weight. Whewell does not succeed in stating precisely what he means when he says that number is a natural measure. It is evident that, for him, there are two meanings involved here. First, it is "just obvious" to him that many natural phenomena have numerical characteristics. The concept of a unit of measurement--*i.e.*, the characteristic of being one--would seem to be basic, even though in nature we may also discover groups of two, groups of ten, and so on. At any rate, it "simply is the case" that most human beings have two ears and ten toes for example. Number, then, is a natural measure in that nature has a quantitative characteristic. Secondly, number is a natural measure "since it measures itself, and does not . . .

require an arbitrary standard."[62] Just precisely what this means, Whewell neglects to state. Part of the meaning, however, is clear. We do not employ something other than number in order to measure number. But just what we are measuring when we are measuring number (as opposed to measuring nature by means of number, as when I say that I have ten toes) Whewell does not make clear. He does assert, at one point, that "number is a modification of the conception of Repetition, which belongs to the Idea of Time."[63] But I do not find this statement very enlightening.

Whewell devotes most of his discussion of measurement to the problems associated with measuring number, space, time, and weight. But he discusses also the problems associated with measuring secondary qualities. Secondary qualities are measured by the employment of scales. Such scales "refer" the qualities to some "definite expression" like space or number. Sometimes, as in the case of the thermometer, the scale can be substituted for the senses. But in other cases, while the senses may be assisted, they are not superseded. For example, in employing the chromatometer, the eye must still determine that any specific colour, "matches" a specific division of the chromatometer. "Colour and sound have their Natural Scales, which the eye and ear habitually apply; what science requires is, that those scales should be systematized."[64]

(iii) The Explication of Conceptions

I shall discuss the explication of Conceptions more fully

when I come to the section on the method of deduction. For the present a few brief remarks will suffice.

It has already been stated that Ideas are certain comprehensive forms of thought, such as space, time, number, cause, resemblance, and the like, which we apply to the phenomena we perceive and think about. Conceptions are special modifications of the Ideas. A circle, a square number, an accelerating force, a genus, provide examples of Conceptions.

Whewell shows, with a wealth of documentary material, that advance in science has had to wait upon the clarification of the Conceptions which the sciences involve:

> The struggles by which philosophers attained a right general conception of plane, of circular, of elliptical Polarization, were some of the most difficult steps in the modern discoveries of Optics. A Conception of the Atomic Constitution of bodies . . . is even now a matter of conflict among Chemists . . . . The imperfection of the science of Mineralogy arises in a great measure from the circumstance, that in that subject, the Conception of a Species is not yet fixed. In physiology, what a vast advance would that philosopher make, who should establish a precise, tenable, and consistent Conception of Life![65]

To clarify these Conceptions, then, is an important function of the scientific process.

One opinion from which Whewell never varies is that in the matter of applying Conceptions to nature, it is possible to be right and it is also possible to be wrong. Man is the interpreter of nature, not merely the observer. But man is not the legislator of nature.

The explication of a Conception usually takes the form of an attempt to define a term, such as "uniform force,"

"genus," and so on. Although it is the case that Conceptions are "unfolded out of" Ideas, we cannot usually arrive at a proper definition by consulting our Ideas alone. What science wants are terms defined in such a way that a proposition containing the term defined will be true of experience. Thus we find, in Whewell's <u>History of the Inductive Sciences</u>, that the endeavour to define "uniform force," for example, was always associated with the intent to assert that gravity is a uniform force.

However, important as definition is, it need not precede the discovery of true statements. Sometimes it does precede, as in Kepler's discovery that the orbit of Mars is an ellipse. But sometimes the true statements come first. Galileo, for example, discovered many truths about the behaviour of bodies when they are moving downwards on an inclined plane, even though he was not certain whether he was discovering truths relevant to momentum, or force, or something else.

The explication of Conceptions, therefore, requires the discovery of true statements, as well as the definition of terms:

> The business of Definition is part of the business of discovery. When it has been clearly seen what ought to be our Definition, it must be pretty well known what truth we have to state. 66

> In establishing a proposition by Induction, the definition of the idea and the assertion of the truth, are not only both requisite, but they are correlative. Each of the two steps contains the verification and the justification of the other. The proposition derives its meaning from the definition; the definition derives its reality from the proposition. If they are separated, the definition is arbitrary or empty,

the proposition is vague or verbal."[67]

(iv) The Induction of Laws of Phenomena

> When facts have been decomposed and phenomena measured, the philosopher endeavours to combine them into general laws, by the aid of ideas and conceptions . . . . In this task of gathering laws of nature from observed facts . . . the natural sagacity of gifted minds is the power by which the greater part of the successful results have been obtained; and this power will probably always be more efficacious than any Method can be. Still there are certain methods of procedure which may in such investigations give us no inconsiderable aid. [68]

Granting, then, that there is no formula of induction, i.e., no set of rules which, if followed, will always yield laws of phenomena, nevertheless, by subdividing the inductive act into three steps we can, as Whewell says, discover "certain methods of procedure" which may be of "no inconsiderable aid." These three steps he calls "the Selection of the Idea," the "Construction of the Conception," and the "Determination of the Magnitudes." The construction of the Conception and the determination of the magnitudes have been discussed above. What remains to be discussed is the method of selecting the Fundamental Idea. On this point Whewell offers three suggestions.

(a) The first suggestion is simply what common sense might propose, namely that we "take" some Idea and "try" it. We observe, for example, that there are cold years, hot years, dry years, wet years. And we may decide to see if we can discover a law of these observations. They do not relate, in an obvious way, to any particular Idea. But a

number of Ideas occur to us which might be relevant. We might refer the observations to the Idea of time, by introducing the Conception of a cycle. Or we might refer them to the Idea of force, by introducing the conception of the moon's action. Or we might try to relate them to the Idea of mutual action, by introducing the conceptions of thermotical and atmological agencies operating between different regions of the earth.[69]

> It may be asked, How are we to decide in such alternatives? How are we to select the one right idea out of several conceivable ones? To which we can only reply, that this must be done by trying which will succeed.[70]

(b) The second "method"--"rule" would perhaps be a better term--which Whewell suggests for the selection of the appropriate Idea is that the Idea and the facts must be homogeneous. It was pointed out above that the first step in induction is the decomposition of facts. If the facts are to be colligated by an Idea then the Idea must be of the same nature as the Conceptions employed to accomplish the decomposition of facts. Thus, if facts have been observed and measured in terms of space, then the Idea of space must be employed to accomplish their colligation.

(c) The third method--and, again "rule" is a better term--is that the Idea must be tested by the facts. Although Whewell's account of this method is somewhat obscure, he seems to me to be asserting a method of proof rather than a method of discovery. Having determined to try to relate the facts to some selected Idea, we apply Conceptions derivative from the Idea to the facts. If, as a result of this process

we succeed in discovering a law of the phenomena, then we know that we have selected the right Idea. Yet, I agree with him that there is an element of discovery here. I discover that I am employing the right Idea, when I prove that I am. It is only after I have discovered the law that I "discover" that I was employing the right Idea. The oft-cited example of Kepler's discovery of the orbit of Mars illustrates this point very nicely.

The ultimate goal of the inductive sciences is to discover the causes of phenomena. But before we can hope to know the causes of phenomena, we must first discover the laws of phenomena. Frequently, therefore, the immediate goal of induction is to discover such laws. As Whewell says, it is necessary to learn the "how" of things, before attempting to learn the "why" of things. Yet to know why phenomena occur is a goal of knowledge, and it is a goal which induction can reach. "Newton . . . discovered, not merely a law of phenomena, but a <u>cause</u>; and therefore he was the greatest of discoverers."[71] Whewell accordingly rejects the view that the inductive sciences should restrict themselves to discovering the laws of phenomena in Comte's sense of laws of the succession and co-existence of phenomena. Nevertheless, it is the case that the search for causes, if it is to be successful, must be preceded by the search for laws. For example, the ancients made the mistake of trying to begin with a knowledge of the causes of things, and, largely because of this error, achieved very little in their inquiries into nature. To dis-

cover the causes of things must be the end, not the beginning, of scientific inquiry:

> The Aristotelian maxim, which sounds so plausible, and has been so generally accepted, that 'to know truly is to know the causes of things,' is a bad guide in scientific research. Instead of it we might substitute this: that 'though we may aspire to know at last why things are, we must be content for a long time with knowing how they are.'[72]

Whewell's view that our knowledge of the laws of nature results from the colligation of facts is, perhaps, the best known element of his philosophical system, and is one of the main points of disagreement between Whewell and John Stuart Mill. The essential item involved in Whewell's doctrine on this point is that whenever a law of nature is discovered, the "discovery" really consists in the relating of a number of observations by means of an act of the intellect. This act is something introduced into the situation by the mind and is not among the <u>given</u> facts, even though the law so discovered may actually have an objective counterpart in the physical universe:

> 'But,' he /(Mill)/ adds, 'a conception implies and corresponds to something conceived; and although the conception itself is not in the facts, but in our mind, it must be a conception of something which really is in the facts.' But to this I reply, that its being really in the facts does not help us at all towards knowledge, if we cannot see it there.[73]

From the standpoint of the person performing the induction, then, the induction first takes the appearance of a supposition, a guess, an hypothesis. And, according to Whewell, the working out of a successful induction involves three steps: the selection of the Idea appropriate to the

data, the construction of the conception which is to be superimposed upon them, and the determination of the magnitudes, relevant to the conception, which the phenomena exhibit.

The conceptions which occur must be tested by reference to observation and experiment, because what we want are not only hypotheses but true hypotheses. And, in order to meet the requirements of truth, as Whewell conceives it, an hypothesis should conform to the following tests. It should agree with the facts. It should make possible accurate prediction of untried cases. It should tend to confirm other inductions. And the general scheme of inductions should show a progressive tendency towards simplicity and unity. This tendency towards unity is furthered when some momentous induction, which consists in a colligation of numerous inductions of a lesser order, is formulated. Such inductions give rise to what Whewell calls epochs, and thus we find him speaking of the epoch of Copernicus, the epoch of Kepler, the epoch of Newton, and so on.

(v) The Induction of Causes

The induction of causes occurs when we relate phenomena to the Idea of Cause. As far as the method involved here is concerned, it is the same as that which is employed in the induction of laws. And since this method has been outlined above, there is no need to repeat here what has been said there. But it is necessary to give some idea of what Whewell means by "cause."

In general, when he talks about cause he has in mind

force. Thus in illustrating the incuction of causes, for example, he cites polarity as a cause. And after "Polarity" he puts in parentheses "Polar forces." But he does not always equate cause with "force." Sometimes Force is said to be a conception derivative from the Idea of Cause. (And one Conception derivative from the Idea of Cause--inertia[74]--is completely opposed to the meaning which "force" has for me, at any rate.)

Although Whewell does define "cause" in a number of places,[75] in general he prefers to say that, for purposes of science, the Idea of cause is expressed, not in a definition, but in three axioms. These are: (a) every event must have a cause; (b) causes are measured by their effects; and (c) reaction is equal and opposite to action.

Whatever "cause" may mean, it is at any rate his belief that the discovery of causes is the ultimate goal of science on its inductive side.[76] In a section entitled "Of the Induction of Causes" Whewell discusses very briefly the induction of the following causes: substance, force, polarity, and a Supreme Cause.[77] But elsewhere Whewell speaks also of other kinds of causes.

If we borrow the terminology from Aristotle--as Whewell appears to do--we may speak, in Whewell's case, of a First Cause, a material cause, a formal cause, and an efficient cause. Whewell emphasizes, especially in relation to the biological sciences, the necessity of positing final causes--*i.e.*, ends in nature. And he also speaks of a final Cause--

i.e., the actualization of the intent of the Creator, an end in view which the Creator had for the whole of creation. The meaning of "cause" is not, as far as I can see, the same in all the above cases.

In a passage quoted earlier, The Idea of Cause was said to "include" Force and Matter. It seems to me, then, that we may speak of efficient and material causes. And while force and matter are related, for Whewell, they are not identical. Our Ideas must also be considered as causes, at least in the sense of being conditions of experience. Ideas function as formal causes. As indicated earlier, we may speak of final causes in two senses. (a) There are final causes in the sense of ends in nature. No one can doubt, according to Whewell that the ear was "designed for hearing" and that hearing is its natural end. (b) The universe at large may be thought of as having a final Cause, an end in view. There is also a First Cause, for Whewell, both in a temporal and in a non-temporal sense of "first." All the palaetiological sciences, we are told, refer back to a cause which, temporally, was first. But there is also a First Cause in the sense of "something ultimate":

> In the utterances of Science, no cadence is heard with which the human mind can feel satisfied. Yet we cannot but go on listening for and expecting a satisfactory close . . . . The idea of something ultimate in our philosophical researches, something in which the mind can acquiesce, and which will leave us no further questions to ask of _whence_, and _why_, and _by what power_, seems as if it belonged to us, as if we could not have it withheld from us by any imperfection or incompleteness in the actual performances of science. 78

If "cause" does have the same meaning in all the above cases--and it is my judgment that Whewell tries to give it the same meaning--the common meaning is that of power. At one point Whewell defines "cause" as follows: "By cause we mean some quality, power, or efficacy, by which a state of things produces a succeeding state."[79] But the definition is unsatisfactory. It is difficult to determine whether "quality," "power," and "efficacy" are intended to be synonymous. Furthermore, it is difficult to conceive a formal cause as a quality, power, or efficacy by which a state of things produces a _succeeding_ state.

In a passage apparently directed against Hume's analysis of cause, Whewell rejects the possibility that events may have no cause, and also rejects the definition of cause as merely that event which invariably precedes another.[80] Whewell's argument here is essentially an appeal to "what everyone knows," namely, that by cause we mean not only that event which invariably precedes another, but that event which _must_ precede the other: "Why must it? Where is the necessity? That there is such a necessity, no one can doubt."[81] Whewell's answer to his question as to the origin of the axiom that every event must have a cause, _i.e._, that for every event there is some specific kind of event which must precede it, is to say that this is an axiom derived from our Idea of cause. He accepts Hume's argument that the term "cause" does not name any datum of external sense. "Experience cannot conduct us to universal and necessary truths:- not to universal, because she has

not tried all cases:- not to necessary, because necessity is not a matter to which experience can testify."[82] But, although he agrees that experience of phenomena cannot provide the necessary information he insists that we do have a concept of cause as the "quality, power, or efficacy," necessary to the appearance of the effect. That every effect must have a cause, in the above sense, he calls an axiom. And although he accepts Hume's argument that sense and experience cannot be the ground of the axiom, he refuses to agree with Hume that we, therefore, have no knowledge of such an axiom.

But the ground of this knowledge is not set forth by Whewell in as straightforward a manner as it should be. And his arguments in support of it tend to exhibit an ambivalence. For example, having discussed briefly what Hume and Kant have to say on the nature and extent of our knowledge of cause, Whewell makes the following statement: "Now the essential point in the view which we must take of the idea of cause is this,--that our view must be such as to form a solid basis for our knowledge."[83] He then asserts that in the mechanical sciences we have universal and necessary truths concerning the subject of causes. And he goes on to say that since any view which refers our belief in causation to sense experience cannot explain the possibility of such universal and necessary truths, it is necessary to assert that the mechanical sciences rest not upon generalizations from sense experience, but upon axioms which flow from the activity of the mind in examining the Idea of cause. Such activity cannot, I should say, be

called induction if Newton's discovery of gravity be taken as a model of the discovery of a cause by means of induction. In the mechanical sciences as they are described above, our knowledge of causes is not the imposition of Conceptions upon facts, but, instead, the unfolding of Conceptions out of Ideas.

It is also difficult to be certain how, according to Whewell we arrive at our knowledge of a First Cause and of a final Cause. There is also difficulty in attempting to regard our knowledge of formal causes as being, in all cases, the result of induction. I shall deal with these briefly in the order cited.

Whewell's use of the term "cause" in the expression "First Cause" is consistent with his usage of the term elsewhere in that a First cause enables us to understand why things occur. Furthermore, Whewell clearly regards the First Cause as a force, the force necessary to account for the appearance of the universe as its effect and the force necessary to maintain the universe in existence. When expressed in this way a First Cause seems to be a legitimate scientific concept--an ordering of the facts of experience by means of an act of the intellect. But Whewell frequently equates this scientific concept with the religious concept of a Creator. And I, for one, cannot understand how knowledge of a Creator can be arrived at by means of induction. If there is an act of induction involved here, then the Creator must be either an Idea or a Conception imposed upon the objects of percep-

tion. Whewell cites a First Cause as a Fundamental Idea. But he does not list a Creator as a Fundamental Idea. And I do not understand how a Creator might be unfolded as a Conception out of the Idea of First Cause. Whewell's proof for the existence of a First Cause, in either of the above senses, appears to me to be different from the kind of verification to which he appeals in the case of truths arrived at by means of induction. The most persuasive of Whewell's proofs for the existence of a First Cause is his claim that a First Cause is an "assumption" which satisfies a demand of reason.

Similarly, final causes, and a final Cause, are not, according to him, Conceptions superimposed upon "facts." They are, instead, "necessary assumptions" which enable the mind to comprehend how certain events can be brought within the framework of knowledge.

> It has been held, and rightly, that the assumption of a Final Cause of each part of animals and plants is as inevitable as the assumption of an efficient cause of every event. The maxim, that in organized bodies nothing is <u>in vain</u>, is as necessarily true as the maxim that nothing happens by chance. I have elsewhere shown fully that this Idea is not deduced from any special facts, but is assumed as law governing all facts in organic nature, directing the researches and interpreting the observations of physiologists.[84]

(vi) <u>The Application of Inductive Discoveries</u>

The application of truths found out by induction is only in certain cases a part of science. Where the application consists in the verification of the discovery by additional experiments and reasonings, this is a part of science. Further-

more, the extension of the induction to new cases not taken into account by the original discoverer is also properly called scientific activity. But the application of inductive truths to secure a practical end--*i.e.*, what is today called technology--Whewell calls art, and says that this is not a part of science.

8. Induction as a Method of Establishing Definitions

Whewell subscribes to the view that not only words but also things can be defined. What he apparently means by defining things is setting forth in words an account of what used to be called the essence of a thing.[85] He speaks, for example, about defining man and offers the definition, "Man is a reasonable animal." And speaking of dogs he says, "Anyone can make true assertions about dogs, but who can define a dog?"

In his attempt to explain the method of arriving at definitions of men, dogs, and the like, Whewell vacillates between two positions. He sometimes holds that such "definitions" are the result of induction. But at other times he holds that such definitions are made possible by an intuitive recognition of the natural classes which nature exhibits.

(a) Definitions of Objects the Result of Induction

A statement which asserts the essence of the object Whewell calls a definition. Sensible intuitions are the evidence for the existence of the object. When we make ourselves the object of inquiry, intuition not only supplies us

with the evidence for our existence, but also supplies us with our nature. But where our attempt to discover the nature of external objects is concerned, intuition will not suffice. We intuit representations of the object, not the object itself. In order to get at the nature of the object from the empirical evidence, we must employ induction. That is to say, we must unite the representations by means of an act of the intellect. The ultimate goal of science is to know the object, not in terms of its sensible representations, but in terms of its formal qualities. Once this goal is reached the science may be based upon intuitively known axioms. But the road to this goal is via inductively established truths. And some of these truths, namely those which state the essential characteristics of objects, are neither laws nor causes. They are, instead, definitions. We may, therefore, properly say that some definitions are arrived at inductively. Not all definitions are so arrived at, however, as I pointed out, above, under "The Explication of Conceptions." But some definitions necessarily are arrived at by means of the colligation of facts. Induction is, therefore, a method of establishing definitions.

As an example, let us consider a botanist who is attempting to classify plants. In Whewell's opinion, the botanist's desire at this point is to discover the definitive characteristics of the plants before him. He is not attempting to define the words "rose," "narcissus," and the like, but the genera _Rosa_, _Narcissus_, and the like. How does the botanist proceed?

First he "decomposes the facts," _i.e._, breaks down the plants before him into smaller units--pistil, stamens, petals, _etc_.--and then attempts to relate these parts to a fundamental Idea. Perhaps he selects the Idea of likeness. (This would be selected tentatively, _i.e._, as a hypothesis.) If he selects this Idea he would begin to arrange the plants before him in terms of likeness. Some plants have the same number of stamens, some have the same number of petals, and so on. Eventually, the botanist will decide that certain likenesses are definitive of a genus which he may then name _Rosa_: A rose is a shrub having rodlike, prickly stems. Its flower has five petals. And so on. To say that the roses which he has examined have the above characteristics merely describes certain plants which he has examined. To say that all roses have such characteristics, and that such characteristics are definitive of roses, constitutes a definition of a rose.[86] That such a "definition" is an induction is shown by such considerations as (1) it unites a number of sensible representations by means of an act of the intellect; (2) the representations are united by relating them to an Idea; (3) the "definition" is a general, as opposed to a particular, statement, and applies to all roses; and (4) it "proceeds" from the known--the roses examined--to the unknown--the roses not examined.

(_b_) _Definitions of Objects the Result of the Intuition of Natural Classes_.

On the basis of the above view it might have to be al-

lowed that we impose a classification upon nature. But if there are natural classes,--and Whewell thinks there are--then the proper job of the classificatory scientist is to discover these classes, not create them. To the extent that these classes are discovered, it would seem that we recognize them directly and intuitively:

> If we had not the power of perceiving in the appearances around us, likeness and unlikeness, we could not consider objects as distributed into kinds at all. 87

Whewell's usage in the above quotation asserts his realistic view that natural objects in themselves exhibit certain definitive features, and that it is these definitive, or essential, characteristics that must appear in a proper "definition" of the object. On the basis of such a view a good "definition" becomes synonymous with a correct description. The "definition" of dog, and the description of a dog in terms of the essential, or defining, characteristics of a dog, will be identical. This seems to Whewell an essential assertion if we are to make possible reasoning concerning things. Without the persuasion that there are characteristic marks by which things can be defined in words, our deductions remain purely formal and without any empirical reference. If, for example, we wish to reason concerning the relations between men and animals it is necessary that "man" and "animal" be defined in such a fashion that such reasoning can be performed. We, therefore, necessarily assume the possibility of such definitions. And, in order that our reasonings may relate to the

phenomenal world we assert the existence of such defining characteristics. For this reason, "We must in all cases of doubt or obscurity refer, not to words or definitions, but to things."[88] From this we may conclude that the task of the scientist here is first to discover the things and then name them.

There are, then, in Whewell, two opposing views about our definitions of objects. One is that a definition represents an act of induction in which the mind imposes a form upon sensible representations. The other is that the form is given to the observer along with the sensible representations, with the result that the definition is a description.

(c) Three Attempts to Resolve the Above Opposition

Whewell clearly recognizes that the above views are opposed. How this opposition may be resolved is discussed by Whewell in three different ways which I will now discuss *seriatim*. His first attempt at a resolution is to show that the imposition of formal qualities upon experience precedes the scientific act of classification. Scientific classification merely employs formal qualities already present in the objects it classifies. It is true that objects are not given as classified. Their classification does, therefore, require further Idealization--*i.e.*, induction. But it does not employ any Ideas which are not already present in the objects prior to their classification. We have a theory to the effect that the objects which the non-scientific mind perceives are so

perceived as the result of forgotten, and unrecallable, colligations of facts. The facts in this case would be non-cognitive and precognitive entities called sensations. In setting up classes of objects the task of the scientist is to discover the manner in which objects have been formally structured by the perceiver. In defining classes his task is to discover a structure which has already been imposed. The structure which has already been imposed is largely unconscious. We non-scientists can tell a rose from a daffodil, a cat from a dog, and so on. But we are not very articulate when it comes to explaining what distinguishes one from the other. In the case of the scientist he, qua scientist, makes articulate a structure which has already been imposed by him in an unscientific way.

The scientist begins with perceived objects. These are objects by virtue of the fact that they already possess a formal structure. The scientist, therefore, classifies objects in terms of this formal structure. It is quite true that objects possess this structure by virtue of antecedent acts of induction. But the classificatory process takes place after these unconscious acts of induction have been performed. Even though classification represents a further idealization, it always employs only formal properties already present. Classification is, therefore, not in all cases a new act of induction, but an organization of "old" inductions. Since it does not impose new formal properties upon the objects it is not induction. Induction, employing sensations as data,

yields objects of perception. Classification, employing objects as data, yields not new, or altered, objects, but a science of objects. Some examples will help to clarify this contention.

Rudimentary classifications involve only the Idea of likeness as, for example, classification by type. Classification by type occurs when one takes an individual as representative of a species. A particular plant which I now hold in my hand becomes the basis of a classification when I decide that I shall call it, and all plants "like" it a rose. Did I create this classification or discover it? The answer is that I did both. I "created" the rose. And I created the class in that I determined to take this particular plant as a type. I discovered it in that I found out that there are a lot of plants like the one I am now holding. In this sense the class is a natural class.

Whewell has three major arguments in support of the existence of natural classes. (a) The attempt to classify things in nature--as opposed to experience--presupposes natural genera. (b) "The manner in which Genera have been established proves that they are regulated by the principle of being natural, and by that alone. For they are not formed according to any *a priori* rule."[89] They are, in other words, discovered *a posteriori*. (c) We are able to observe--in botany and zoology at any rate--that some characteristics are necessarily related. On the basis of such an observation we can attempt to classify

things in nature by employing not simply the Idea of likeness, but also the Idea of affinity. Here classification proceeds not by taking some individual as a type, but by employing new inductions. Whewell, however, will not allow that these inductions in any way change the objects.

Although for purposes of consideration, we may separate the functions of living things, these cannot be separated in nature. Certain functions "go with" certain other functions. And modifications in the one are accompanied by modifications in the other. Frequently when we classify things in accordance with one function, and then classify them in accordance with another, we find that the two classifications coincide. In such a case, Whewell believes, we have discovered a natural classification. He quotes, in support, Decandolle on this point. It has been found that if plants are classified as monocotyledonous and dicotyledonous--a classification established in terms of their reproductive organs--the same results are achieved as when they are classified as endogenous and exogenous--a classification established in terms of the process of nutrition. "Thus," Decandolle, asserts,

> the natural classes founded on one of the great functions of the vegetable are necessarily the same as those which are founded upon the other functions; and I find here a very useful criterion to ascertain whether a class is natural: namely, in order to announce that it is so, it must be arrived at by the two roads which vegetable organization presents.[90]

But plants are not originally "given" to the perceiver as monocotyledonous. That there might be such a class as monocotyledons is, at first, an hypothesis, an induction.

Prior to this it would have to be known that certain parts of plants are reproductive organs. To know that a plant has reproductive organs itself involves an act of the intellect. But once it becomes known that plants do have a sexual apparatus, then this becomes part of the realm of fact. However, at this stage it is still not evident to the observer that plants may be classified by reference to their reproductive organs. To classify them in this way requires a further act of the intellect. Such an act is an induction, and, to this extent, the classification is imposed upon nature. But this induction did not create the sexual organs of plants. Instead, by means of the induction it was discovered that plants could be classified in terms of a structure which pre-existed that particular act of induction which accomplished the classification.

There are, in my opinion, two major weaknesses in Whewell's views concerning classification of the objects of perception. (a) The first is that in his account of the classificatory sciences Whewell seems to use "induction" differently than he does in his discussion of the other sciences. In his discussion of the other sciences induction is a method whereby Conceptions are imposed upon data. But in his discussion of the classificatory sciences induction appears to be a method whereby a pre-existent order is discovered. (b) The second weakness in his account is that he confuses the particular with the universal. It is obviously the latter--man, dog, rose, etc.,--that he is trying to "define." But he seems to think

that his task--or, at any rate, the task of the scientist--is to define the former. Mill asserts that the problem at this level is to determine what a general name denotes. Whewell asserts that the problem is to determine what the object denoted *is*.

The second way in which Whewell tries to resolve the apparent discrepancies in his account of how we come to know that there are kinds of natural objects, is conducted in terms of the "principle of intelligibility." It would seem to be his view that there are *many* principles of intelligibility. Nevertheless he sometimes speaks of *the* principle of intelligibility. The principle may be formulated in two ways, one of them very general, the other more specific.

Speaking very generally, the principle of intelligibility is that words must be defined in such a way that intelligible discourse becomes possible. The grounds of the possibility of intelligible discourse are many. But only one of these concerns us here, namely, that assertions concerning objects must be possible. These assertions are of two kinds, particular and general. But particular assertions--*e.g.*, This tree has green leaves--always imply the possibility of general assertions. To speak of "this tree" for example, implies that I can make general assertions about trees. Trees are fixed in the ground, have a solid stem, branches, leaves, and so on. The condition of intelligibility, therefore, is that general assertions shall be possible. Fully formulated, this principle is that "the condition of the use of terms is the

possibility of general, intelligible, consistent assertions."[91]

Speaking more specifically, the principle of intelligibility is that a word shall name one thing, and one thing only. When expressed in this form the principle of intelligibility may be rephrased as the principle of unity. And, although many words obviously have many meanings, this consideration is irrelevant. The principle of intelligibility still remains that when we use a word we can indicate what, in the context in which we use it, the word names. A corollary to the principle--actually, Whewell says it is the same principle seen from the objective point of view--is that there should be "things" which are "one." Unless there are such things, then a word cannot be the name of "one thing." And unless names can name one thing, they are of no use.

> A word which expressed a mere wanton collection of unconnected attributes could hardly be a word; for to such a collection of properties no truth could be asserted, and the word would disappear for want of some occasion in which it could be used.[92]

Since what we are aiming for in science is general assertions, our chief interest lies with general names. Such names refer to many individuals which we associate because of certain resemblances among them, and because of certain permanently connected properties. Such collections of individuals are called "kinds," "sorts," "classes."[93] "Man," "animal," "living thing," are each a name of a kind. We also note that "living thing" includes men and animals, but excludes stones. "Body" would include all four.

To the question, "What justifies such names?" Whewell

gives two answers. (a) They are justified if we can use them to express true propositions, and (b):

> When we have such a Series of Names and Classes, we . . . take for granted irresistibly that each class has some character which distinguishes it from other classes . . . . We ask what kind of beast a dog is; what kind of animal a beast is; and we assume that such questions admit of answers;--that each kind has some mark or marks by which it may be described . . . . We entertain a conviction that there must be, among things so classed and named, a possibility of defining each.[94]

Now we must ask, What is the ground of this conviction? Whewell's answer is as follows:

> Our persuasion that there must needs be characteristic marks by which things can be defined in words, is founded on the assumption of the necessary possibility of reasoning.[95]

If we are to reason, not about words, but about men and animals, for example, it is necessary to "define" man and animal in such a way that our reasonings will refer to them. If we "define" animals as "beings impelled to action by appetites and passions" and if we "define" man as an animal, we may deduce that man is impelled to act by appetites and passions.

> And if we add a further definition, that 'man is a reasonable animal,' and if it appear that 'reason implies conformity to a rule of action,' we can then further infer that man's nature is to conform the results of animal appetite and passion to a rule of action.
>
> The possibility of pursuing any such train of reasoning as this, depends on the definitions, of animal and of man, which we have introduced; and the possibility of reasoning concerning the objects around us being inevitably assumed by man from the constitution of our nature, we assume consequently the possibility of such definitions as may thus form part of our deduction, and the existence of such defining characters.[96]

The third way in which Whewell relates his view that

there is a nature which we discover directly and a nature which we assert hypothetically on the basis of inductions, is not unlike the account above. It differs only in that it gives expression to a more obviously pragmatic approach both to nature and to knowledge. The pragmatic element is clearly suggested above, and I shall not elaborate it here at any great length. It is certainly implied in the preceding account that we first decide what we want--in the above case we want to reason deductively about nature--and then we set about discovering means to this end. If the means "work," they are justified.

Whether the pragmatism in the above account is conscious or not, I do not know. But there are certainly instances of conscious pragmatism in Whewell. One such instance is a brief discussion which he conducts of the problem of "defining" whales. Is a whale a fish or a mammal? His answer is that a whale is a fish or a mammal, depending on the purpose of the person who is doing the defining. From the standpoint of the fisherman a whale is--i.e., may properly be defined as--a fish. The fisherman's classes of genera include "things I catch for a living." One kind of thing he catches for a living is the whale. And since for him whales, cod, mackerel, and the rest, are defined in relation to his occupation, the whale is a fish. It is something he "fishes" out of the sea. For this reason, says Whewell, it is quite proper to speak of whale fisheries. But in natural history the whale is classified as a mammal, because to do so suits the purposes of the natural historian. The natural historian has decided to classify not on the basis

of what things are, and what things are not, fished out of the sea, but, rather, on the basis of certain permanent collections of properties. Why he does so, and the justification for doing so, is discussed in (2) above.

(d) The Above Opposition not Satisfactorily Resolved.

The material just set forth is compatible with Whewell's assertions elsewhere that definitions are the results of the mind's activity operating in accordance with the principles of unity and intelligibility. But it is clear that in the above three accounts there are elements of two incompletely assimilated views. The one view is that the principle of unity is logically prior to the perception of the object which has been constructed in accordance with that principle. The other is that the object itself is logically prior to the definition of it and, presumably, is therefore also logically prior to the principles of unity and intelligibility.

Are objects given to the scientist, or does the scientist create them in order that he may have an intelligible world? Does the scientist make the world intelligible, or does he discover its intelligibility? Whewell runs into a great deal of difficulty in this area, and I doubt whether his position can be stated in a more consistent way than he himself states it. He really is pulled in two directions. For he has to bring together at least the following claims: (1) objects are the result of the mind's activity; (2) the construction and the naming of objects are interrelated since the name must

stand for some identifiable thing of which meaningful and significant predicates can be asserted; (3) predication and classification are interrelated since true predication is frequently dependent upon proper classification; and (4) all the foregoing are interrelated by means of a correspondence theory of truth. Since in order for a statement to be true it is necessary that the facts have the same relation in a proposition that they have in reality, Whewell's problem seems to be to make (1), (2), and (3) square with the correspondence theory.

Whewell does not make precise enough whether the process of scientific naming rests upon the need for intelligible discourse or upon observational procedures for discovering the ways in which objects and creatures are in fact alike, apart from the demands of intelligible discourse. There is a difference between discourse which is merely intelligible and discourse which, in addition to being intelligible, states what is true of the objective world. His account of naming and classification is vague for the reason that he does not succeed in amalgamating two distinct contentions, the first being that the mind by its own activities introduces intelligibility into experience, and the second being that true scientific discourse is not about experience but about the independently organized and intelligible external world, the preexistent universe of natural objects. For example, his claim that I must make the tree before I can see it is at variance with his claim that in biology, as elsewhere, "there

may be many classifications which are moderately good and natural, but there is only one which is the best and true natural system."[97]

The problem is not too obvious on the level of naming individuals, although it is certainly latent on this level. When I name an animal a "lion" I have to have some sort of reason for doing so. Here, according to Whewell, the main problem is to employ the principles of unity and intelligibility in such a way that I distinguish the lion from the zebra he is devouring.[98] But when he comes to the problems of classification he begins to worry not only about the intelligibility of discourse but also about its truth. What he says about naming at one point seems to make it necessary that truth be derivative from the operation of naming. If, as he says, I group a number of sensations and call the group a tree, then it is a fact that that is what a tree is. I have determined the truth of the proposition, "This is a tree," by my determination to name this particular group of sensations a "tree," with the result that any subsequent worry about whether or not it really is a tree is quite out of place. But elsewhere his view is that the truth of such a proposition as "This is a tree" is dependent upon my having accurately identified a natural class.

Another way in which to frame the above objection would be to say that Whewell makes the principle of intelligibility regulative for discourse but is unwilling to permit it to be regulative of nature. And his problem is that true intel-

ligible discourse concerns nature, and is true by virtue of its conformity to nature. Whewell gets into particular difficulty when dealing with the classificatory sciences, since for these he insists that the Book of Nature is their dictionary. He will permit the orbit of Mars to be something which the mind introduces into the facts and which it (not surprisingly) subsequently discovers. But he cannot bring himself to support an analogous interpretation of the origin of natural kinds.

4. Induction and Axioms

I shall set forth Whewell's views about axioms more fully when I discuss intuition and deduction. But one of his views about axioms is relevant here. Our inductions, whether they yield definitions, laws, or causes, can be checked by a method which I shall call the axiomatic method. This independent check upon our inductions prevents our inductions from being arbitrary, and is a means of verifying them.

We may illustrate this by considering definitions. In the case of the objects of the purely formal sciences as, for example, geometry, definitions are prevented from being arbitrary by the necessity that they conform to the Idea from which they spring:

> We may always find an Axiom which shall take the place of a Definition. If we assume a proper Axiom respecting straight lines, we need no Definition of a straight line. But in whatever shape the principle appear, as Definition or as Axiom, it has nothing about it casual or arbitrary, but is determined to be what it is, as to its import, by the most rigorous necessity, growing out of the Idea of Space.[99]

Descriptive astronomy takes as its subject matter empirical space, and is an inductive science. But its definitions cannot be purely arbitrary, at least where the formal quality of empirical space is concerned, because for the descriptive science we can always, in principle, substitute a purely formal, axiomatic, science. To actually succeed in accomplishing this substitution requires someone of the stature of Kepler. It cannot be done by everyone. But in principle it can be done. Our guarantee that it can be done rests in our metatheory where we show that the formal qualities of perception are derivative from the Ideas and Conceptions of the perceiver. Therefore, we should be able to substitute an axiomatic system for at least the definitions and laws of descriptive astronomy to the extent that its laws are laws of the formal properties of space. If we cannot make the substitution, this assures us that there is something wrong with the descriptive, inductive, science. If we can make the substitution, this assures us that our definitions and laws are "true."

Where the causes investigated by the inductive sciences are concerned, we often find a similar check upon the arbitrariness of the definitions of such causes. It is often possible either to substitute axioms for the definitions or to dispense with definitions and employ axioms in their place. For example, "cause" itself, which is the basic concept of all sciences which investigate causes, is introduced not in the form of a definition but under the guise of a number of

axioms.

Axioms are always present in knowledge even though they may not be explicitly stated. In fact, to discover precisely what the axioms of any given science are often requires a long period of time. To make these axioms explicit it is necessary that the Ideas from which they flow be clearly understood. The criterion of the clarity and distinctness with which these Ideas are understood is that that person has a clear and distinct apprehension of any given Idea if he sees the necessity of the axioms pertaining to it, and if he sees the cogency of the arguments based upon the axioms. In the case of such a person we may say that the truth of the axioms is grasped by intuition. The truth is, however, not grasped in a moment, but, rather, arrived at progressively. This holds true both for the formal sciences and for the empirical sciences.

The axioms are not arbitrary assumptions, and they are not selected hypotheses. They are, instead, propositions which we must see to be universally and necessarily true if science itself is to have the characteristics of universality and necessity. Historically speaking, the axioms known to us have been discovered, if not exactly as a result of, or by means of, at any rate along with, observation and experiment. Yet their truth--their universality and necessity--cannot be verified empirically nor demonstrated syllogistically. Their truth cannot be demonstrated because all demonstration presupposes their being true. And they cannot be proven empirically, because empirical observation brings with it no guaran-

tee that our next observation may not go contrary to the rule established by past observation, whereas axioms are of such a nature that their falsehood is unthinkable.

Once again, in Whewell's account, we observe a certain ambivalence. The ambivalence seems to arise because Whewell has not fully resolved the question, Is the ground of necessary propositions the external world or the demands we set up and require that knowledge fulfil? His analysis begins with kinds of knowledge which are, in his view at least, universal and necessary. And he argues that the possibility of such kinds of knowledge lies in the fact that the same functions, or acts, of the mind which yield objects also yield the sciences. In this way he attempts to justify, by an appeal to experience, but not in the sense of an empirical inquiry into experience, a claim that the axioms which give the sciences their universal and necessary quality, have a ground in experience. We know that the axioms are true because we know that objects of experience will always conform to them. Hence, it is unthinkable, for example, that there should be an event which does not have a cause. But _in itself_ the proposition that some events may be uncaused, or _causa sui_, is not unthinkable.[100] What makes it alien to our thought is that if such a proposition be accepted then science of a universal and necessary character becomes impossible:

> Speaking of the two Principles of Chemical Science,--that combinations are definite in kind, and in quantity,--I had tried to elevate myself to the point of view in which these principles are seen, not only to be true, but to be necessary. I was aware that even the profoundest chemists had not ventured to do this;

> yet it appeared to me that there were considerations which seemed to show that any other rule would imply that the world was a world on which the human mind could not employ itself in scientific speculation at all.[101]
>
> If the Axiom of Substance were not true and were not assumed, we could not have such a science as Chemistry . . . . If the Axioms of Mechanics were not true and were not assumed, we could have no science of Mechanics . . . . It is not any special results of the science in such cases; but the existence, the possibility, of any science, which establishes the necessity of these axioms. They are not the consequences, of knowledge, acquired from without, but internal conditions of our being able to know.[102]

5. Induction as the Idealization of Facts

One way in which induction may be characterized is to describe it as that process which best accomplishes the Idealization of facts. It will be recalled that one formulation of the fundamental antithesis expresses it as the antithesis of Facts and Ideas. Within this formulation we may say that science consists in the ordering of facts by means of Ideas. But before expounding this view it is necessary to clear up two verbal sources of ambiguity.

Whewell clearly distinguishes ideas, as mental events, from Ideas as the universal and necessary forms of perception. From the standpoint of the symbols themselves, the only way in which Whewell's reader can distinguish an "idea" from an "Idea" is that the latter has a capital "I" and the former does not. This would not provide any great difficulty if Whewell consistently capitalized the term when it is intended to signify the universal and necessary forms of perception. (Even if he did so, some difficulty would still arise as, for

example, when "Ideas" occurs at the beginning of a sentence.) But, he is not careful to do this. Obviously, it is rather difficult to prove that he is careless, that in certain passages we should read "Idea" for "idea". Nevertheless, I find it advisable, on many occasions, to read "Idea" where he has written "idea." Sometimes the only justification for doing this is to say that, in the light of Whewell's writings as a whole, it is clear that in such and such a passage "idea" should be "Idea." Yet it is always possible that when he writes "idea" he intends "idea." The reason I sometimes substitute "Idea" where he has written "idea" is to make it possible for me to give a consistent account of his philosophy. And I am aware that in so doing I may be misrepresenting Whewell. His philosophy may not in fact be consistent. My procedure requires me to hold that in certain cases Whewell was simply careless. Yet it may be the case that Whewell was not careless in the sense of forgetting to capitalize "idea" but, careless in the sense that he contradicts in one place what he says somewhere else. I wish, therefore, to acknowledge that I have not, in all cases, stated what Whewell literally says but, rather, have stated what I think he intended to say, what he would have said if he had been more careful in his employment of the terms "Idea" and "idea."

There are numerous passages in which "idea" is the key term. If we interpret these passages in the light of the distinction which Whewell makes between "idea" and "Idea" then such passages express an idealism (with a small "i" which

is neither Platonic nor Kantian.

It is, nevertheless, my view that we cannot learn the nature of Whewell's philosophy by deriving it from what he says about ideas. Ideas, with a small "i", play a significant role. But it is Ideas, with a capital "I", which play the determining role. Without "ideas" there would be no knowledge because we should have no mental life. However, the character of our knowledge is determined by "Ideas" not by "ideas." As Whewell says, "Knowledge consists in applying Ideas to Facts."[103] And if we are to hold consistently to this view I think there can be no question that whenever "idea" occurs in a passage like the following, we must substitute "Idea" if we are to have before us what Whewell intends to say:

> In what has already been said on the History of Ideas, we have seen how each science was in a state of confusion and darkness till the right idea was introduced.
>
> No general method of evolving such ideas can be given . . . .
>
> In most cases of great discoveries, the right idea to which the facts were to be referred, was selected by many philosophers, before the decisive demonstration that it was the right idea, was given by the discoverer . . . .
>
> Although, as we have said, we can give few precise directions for this cardinal process, the Selection of the Idea, in speculating on phenomena, yet there is one Rule which may have its use: it is this:-- The idea and the facts must be homogeneous: the elementary Conceptions, into which the facts have been decomposed, must be of the same nature as the Idea by which we attempt to collect them into laws. Thus, if facts have been observed and measured by reference to space, they must be bound together by the idea of space.[104]

At the beginning of this section I stated that induction may be characterized as that process which accomplishes the Idealization of facts. Here a difficulty like the one just discussed arises. Whewell has different meanings for "idealization" and "Idealization." But he is not always careful to capitalize the "I" when it is "Idealization" that he is talk-about.

"Idealization" means the formal relating of phenomena to the forms of perception; "idealization" means the active relating of phenomena to the forms of perception. The former is a formal property of experience and knowledge. The latter is a power of the mind. Both are necessary if there is to be experience. But each is distinct.

To borrow Aristotelian terminology, we may say that Idealization is the formal cause of knowledge, idealization one of the efficient causes. The two always occur together, of course. But they are distinguishable. In the act of thinking--i.e., in idealization--Idealization is always involved. We do not simply think thoughts. Our thoughts always involve a form which is not a thought, but is an Idea, or Conception derived from an Idea, which gives form to the thought and to the thing thought about. For example, in thinking about the top of my desk, one of my thoughts may be, The top of my desk is a rectangle. "Rectangle" is a Conception. And even though I may have a very rudimentary understanding of rectangles, nevertheless what makes my thought possible is that I am able to unite certain representations with the Conception, "rect-

angle." The act of uniting these components is idealization, idealization being a dispositional power of the mind. The result of uniting these components is Idealization which is the equivalent of knowledge.

> The ancients discovered that the planets revolved in recurring periods, and thus connected the observations of their motions according to the Idea of Time. Kepler discovered that they revolved in ellipses, and thus connected the observations according to the Idea of Space. Newton discovered that they revolved in virtue of the sun's attraction, and thus connected the motions according to the Idea of Force.[105]

> In this progression from fact to theory, we advance (when the theory is complete and completely possessed by the mind) from the apprehension of truths as actual to the apprehension of them as necessary; and thus Facts which were originally observed merely as Facts become the consequences of theory, and are thus brought within the domain of Ideas. That which was a part of the objective world becomes also a part of the subjective world; a necessary part of the thoughts of the theorist. And in this way the progress of true theory is the Idealization of Facts.[106]

For Whewell, the Idealization of facts--the capitalization of "Facts," above, seems to have no philosophical significance whatsoever -- is exactly the same process as the colligation of facts. In other words, the Idealization of facts is simply induction viewed from a particular angle, i.e., is the way in which induction appears to us if we stress the role which Ideas play in induction. Both the Idealization of facts, and the colligation of facts, are nothing other than the mediation of particulars by means of the activities of a subject. The activities of the subject unite that which is apprehended as actual in terms of the universal and necessary forms of thought. Or we may say that the inductive act, whether it be regarded

as the colligation of facts or as the Idealization of facts, consists in the employment of a general conception, which is not given to the senses, but is supplied by the mind, to unite that which is given to perception. This is in keeping with Whewell's oft repeated maxim that man is not the spectator of nature only but also the interpreter.

8. The Verification of Hypotheses

One aspect of Whewell's discussion of induction is the matter of the verification of hypotheses. The tests by which the truth of an hypothesis is established have been indicated above. The criteria he suggests are numerous, but the following seem the most significant. In the first place, when we return to the facts, further observation and experiment should confirm the hypothesis. Secondly, a true hypothesis should enable us to predict future events. Thirdly, any consilience of inductions tends to confirm the inductions involved. Since Whewell seems to be one of the earliest writers to emphasize the importance of the consilience of inductions[107] I shall include a brief quotation from him on this matter:

> I have spoken, in the Philosophy of the Consilience of Induction, as one of the Tests of Hypotheses, and have exemplified it in many instances; for example, the theory of universal gravitation, obtained by induction from the motions of the planets, was found to explain also that peculiar motion of the spheroidal earth which produces the precession of the Equinoxes. This, I have said, was a striking and surprising coincidence which gave the theory a stamp of truth beyond the power of ingenuity to counterfeit . . . . And I will add, that I believe the history of science offers no example in which a theory supported by such consiliences, has been afterwards proved to be false.[108]

Fourthly, if we can draw up inductive tables ranging from inductions of a low degree of generality to inductions of a high degree of generality, with the former being included under the latter, these tables confirm the validity of all the inductions concerned. But perhaps the most important of all tests--although Whewell does not explicitly say that he so regards it--is verification by means of deduction. If, from the inductive generalization, we can deduce specific statements whose truth we have previously determined, then we have most impressive confirmation of the truth of the inductive generalization. The fact that from the Newtonian laws pertaining to gravity we can deduce the Keplerian laws pertaining to the motions of the planets is pretty well proof positive of the truth of the Newtonian laws.

What Whewell means when he says that a law arrived at by induction is true is not easily stated, but I have tried to indicate above,[109] by listing the criteria of truth applicable in such a case, what it may mean. But, in addition to the above criteria, Whewell's theory of truth includes a doctrine which is very difficult to maintain. Truth, instead of being a quality of propositions, begins to take on an existential status, and "the Truth" becomes something towards which we constantly strive and which is at present only partially manifest. I should certainly agree with Whewell that at the moment we are not possessed of the whole truth, but I should mean by this that we have not succeeded in affirming all statements which are true and known to be true. But he

says in addition to this, that the statements we now affirm to be true are only "partly" true. And this I cannot understand. For example, either the planets move in cycles and epicycles, or they do not. I cannot make any sense of Whewell's claim, that the ancients, who held to the theory of cycles and epicycles, were "partly right," especially when he explains his meaning by saying that the ellipses in which some of the planets move are "almost" circles.

7. Three Difficulties Associated with Whewell's View that Induction Equals Idealization

The first difficulty here relates to the nature of the knowledge we have of Ideas. Whewell's argument to the effect that I must have an intuitive knowledge of the Idea of space since otherwise my spatial perceptions would have no form is, I think, not proven. And it seems to me that when he attempts to prove it, he always ends up talking not about the Idea, but about the Conceptions to which, according to him, the Idea gives rise, but which do not exhaust the Idea. Furthermore, his account of the origin of the Conceptions is vague and metaphorical. We "unfold" them out of the Ideas. But what knowledge Whewell has of the Idea of space, for example, over and above the spatial conceptions--triangularity, circularity, tridimensionality, and so forth--he seems quite unable to communicate.

Secondly Whewell asserts that the Idea cannot be fixed in words, from which I infer that we cannot have a precise and determinate knowledge of it. And although this might

seem, as I have elsewhere argued,[110] to make for a certain scepticism, Whewell, by accepting the necessity for, and therefore the reality of, the Idea, turns its "inexhaustibility"--from which we may infer our partial ignorance of it--into grounds for optimism:

> The Idea is disclosed, but not fully revealed, imparted but not transfused, by the use we make of it in science. When we have taken from the fountain so much as serves our purpose, there still remains behind a deep well of truth, which we have not exhausted, and which we may easily believe to be inexhaustible.[111]

In a letter to Herschel, Whewell, in reply to criticism from Mill, discusses the possibility of substituting for the term "Ideas," which had come under fire from Mill, either the phrase "irresistible impulse to generalize" or the phrase "laws of the mind's activity."[112] He is willing to accept the phrase "irresistible impulse to generalize" on the condition that it be recognized that this impulse has to be given form. We do not simply, or merely, generalize. Instead, we always generalize in terms of space, time, number, and the like. And we cannot hold that these forms under which generalization appears are present in the object without presupposing that we ourselves supply them. We find, then, that the inductive propensity <u>alone</u> cannot explain the form which inductive knowledge takes.

The phrase "laws of the mind's activity" is also acceptable provided it be interpreted to mean universal modes of activity. And, in order to discuss these modes it will be found necessary to introduce the Fundamental Ideas and to

maintain that these, and not some kind of empirical evidence, account for the universal character possessed by the sciences. Induction based upon experience--that is to say, based upon that which is given to the senses in observation--may give us general knowledge, but cannot give us knowledge which is universal and necessary. "Experience cannot bestow that universality which she herself cannot have; nor that necessity of which she has no comprehension."[113]

Little is to be gained, then, from the introduction of such terms as "irresistible impulse to generalize," and "laws of the mind's activity," unless we emphasize the forms which the impulse and the activity take. For it is the forms, not the activity, that gives us our chief insight not only into the nature, but also into the possibility, of knowledge. This position does not deny the reality of the external world. Nor does it deny that the objects of experience have a definite structure which experience may, in part, reveal. But it does emphasize that in order to discover the conditions of knowledge it is necessary, for philosophical purposes at any rate, to distinguish Ideas from sensations and ideas, and to give Ideas their due.[114]

But while the above arguments seem to me to have considerable force, nevertheless, as noted above, a great difficulty associated with Whewell's account of Ideas is the problem of making clear just precisely what an Idea is. Whewell calls Ideas "necessary conditions of knowledge," "universal forms of intuition," "inherent types of mental development,"

and "the results of connate intellectual tendencies." And he frequently speaks of them as ultimate elements of knowledge. One might almost call them primitive and irreducible objects of thought on its ideal side. If they were such we might, perhaps, hope to know them in and of themselves as something final and certain, with the result that they would thereby represent points at which thought could rest. However, in one passage at least, while not qualifying the universal and necessary nature of the Ideas, Whewell refuses to commit himself as to whether or not they can be known to be simple and ultimate:

> Fundamental Ideas, as we view them . . . are not necessarily ultimate elements of our knowledge. They are the results of our analysis in so far as we have yet prosecuted it; but they may themselves subsequently be analysed. It may hereafter appear, that what we have treated as different Fundamental Ideas have, in fact, a connexion, at some point below the structure which we erect upon them . . . . But even if this be so, it will by no means affect the validity of reasonings founded upon these notions, when duly determined and developed. If we once adopt a view of the nature of knowledge which makes necessary knowledge possible at all, we need be little embarrassed by finding how closely connected different necessary truths are; and how often, in exploring towards their roots, different branches appear to spring from the same stem.[115]

The above quotation seems to treat Fundamental Ideas as axiomatic propositions, or necessary truths, rather than as subjective entities possessing some sort of existence or reality about which universal and necessary statements are possible. But it is easy to find passages in which Whewell treats Ideas as existents of some kind.[116] And the relation of Ideas as existents to Ideas as self-evident or, at any rate, as intuitively known, truths is not satisfactorily worked out. To the

extent that our knowledge rests on intuitively known axioms, I can understand the possibility of knowledge being determinate and precise, provided the guarantee of the truth of the axioms is not some vaguely glimpsed and imperfectly known Idea. If I must first know the Idea in order to know that some truth, presumably descriptive of the Idea, is indeed true, and if I cannot know the Idea, then, obviously, I cannot know that any given statement which purports to be descriptive of the Idea is a true statement.

I have stated earlier that Whewell's unsuccessful eclecticism makes it difficult to expound his system. One element in his philosophy is a vaguely defined "Absolute." Whewell accepts, in a fashion which is not clear, and on grounds which are not stated, an Absolute, meaning by this a "centre" and a "unity," from which all facts and ideas "flow." This unity Whewell claims we can never know. And, therefore, we can never attain to full knowledge. If one were to extrapolate this notion, it would seem to follow that our knowledge of the objects of perception would be similarly partial. "This dog is black," is not entirely true, because it is not the whole truth about the dog. It is not the whole truth because we do not know how this "truth" is related to the Absolute. But sometimes Whewell takes a much different view of things, and then our knowledge of objects is held by him to be complete and certain, and, in the realm of science, some scientific conclusions are undeniably true. And we may hope to show the possibility of equally certain statements in other

areas, e.g., ethics. However, although Whewell vacillates concerning the degree of truth which characterizes both propositions about particulars and propositions which set forth the end products of induction, the same degree of truth applies to both. Since, for Whewell, both the senses and the intellect are involved in all instances of knowing, knowledge is, for him, "all of a piece." It is, then, not a great problem for Whewell, which is the more accurate, sense knowledge of particulars, or intellectual knowledge consisting of the colligations of facts. Each of these is reflected and included in the other. What we say about one, we must say about the other. If one is certain, the other is certain. But if one is "partial," so is the other. This is a problem for philosophy, not science, to decide. But Whewell cannot quite make up his mind what his philosophical position here is.[117]

B. The Method of Deduction

1. Deductive Reasoning and Syllogistic Reasoning

I shall give only a very brief account of Whewell's views concerning the method of deduction because, on this subject, he has nothing original to say. Deductive reasoning for him is syllogistic reasoning. And he accepts, uncritically, Aristotle's exposition of this method.

> Each step of geometrical, and all other demonstrative reasoning, may be resolved into three . . . clauses . . . ; and these three clauses are termed respectively, the major premiss, the minor premiss, and the conclusion."[118]

He praises Mill for having made clearer than Aristotle does what it is that deduction accomplishes. And he accepts Mill's conclusion that the syllogism produces no new truths. "However far we follow . . . deductive reasoning, we can never have, in our conclusion any truth which is not virtually included in the original principles from which the reasoning started."[119]

Whewell's view of deduction is, then, to employ a popular metaphor, that we "unfold" the conclusion out of the premises which "contain" it.

2. The Elements of Deduction

There are according to Whewell three elements in deduction: axioms, definitions, and syllogisms. Deduction "begins with" axioms and definitions and it proceeds to conclusions by employing "rules for determining in what cases pretended reasonings are and are not demonstrative."[120] These "rules" are derived from logic and are known as the valid forms of the syllogism. The other deductive sciences, however, are not simply extensions of logic. Instead, the other deductive sciences, e.g., the mathematical sciences, employ logic. Whewell observes, in passing, that "the peculiar habits which enable any one to follow a chain of reasoning are excellently taught by mathematical study, and are hardly at all taught by logic."[121] What he says about deduction is usually illustrated by references to mathematics and especially to Euclidean geometry.

All the conclusions which occur in the science [of

> geometry_7 follow purely from those first principles /--axioms and definitions--/ of which we have spoken, --that each proposition is rigorously proved from those which have been proved previously from such principles;--that this process of successive proof is termed Deduction;--and that these rules which secure the rigorous conclusions of each step are the rules of Logic. 122

Whewell, in other words, will not allow that the certainty which mathematical reasoning provides is simply that *if* the premises are assumed *then* the conclusions follow. Instead he seeks to show that the conclusions are true *because* the premises are true.

> We cannot conceive or perceive objects at all, except as existing in space; we cannot contemplate them geometrically, without conceiving them in space which is subjected to geometrical conditions; and this mode of contemplation is . . . analysed into definitions, axioms, or both.
>
> The truths thus seen and known, may be said to be known by intuition. 123
>
> /The/ axioms /of geometry/ are stated in the beginning of our Treatises, not as something which the reader is to learn, but as something which he already knows . . . . The student's clear apprehension of the truth of these, is a condition of the possibility of his pursuing the reasonings on which he is invited to enter.124

Whewell says very little about the nature and role of postulates. But his general position here is that there is no place in a deductive system for postulates, *i.e.*, for statements which can merely be presented and accepted on a "let it be granted" basis. Such statements, if allowed, would give the deductive sciences a hypothetical character. And Whewell is firmly convinced that the reasoning encountered in a deductive science is necessary, not hypothetical.

3. The Role of Deduction in Science

(a) A Disagreement with Aristotle

Although Whewell claims to say nothing original about the procedure actually involved in deduction, he does have some original things to say about the role of deduction in science. Some of the premises with which deduction begins are supplied, Whewell claims, by induction. But he disagrees with Aristotle as to the character of these premises. This disagreement is worth elaborating. If it is the case that deduction "unfolds" a conclusion out of premises, it is important to understand the nature of that which is "unfolded."

Whewell's account of Aristotle's theory of induction is that induction occurs when by means of one extreme term we infer the other extreme to be true of the middle term. For example, we know that certain animals such as the elephant, the horse, and the mule, are long-lived. We also discover that these animals have no gall bladder. From these two sets of fact we infer, by induction, that all animals which have no gall bladder are long-lived. However, in Whewell's opinion, the inductive act is not of this sort. Aristotle, Whewell claims, begins with the relevant conceptions already given, whereas in Whewell's view the truly inductive act is the discovery of the relevant conception which permits a colligation of the facts. Another example intended to establish the same point is the discoveries of the laws of the planets. To say that the orbit of Mars is elliptical, the orbit of Venus is

elliptical, and the orbit of the earth is elliptical, and that therefore all the planets move in ellipses, is not, according to Whewell, an instance of induction. The induction consists in seeing the concept of the ellipse as significant in this context. In Platonic terms, the true problem for induction is to find the one in the many. To this extent, Whewell asserts, Plato had a truer concept of science than had Aristotle.

(b) Deduction as a Method of Corroborating Induction

Unless from an empirical generalization one can deduce true statements of a lesser order of generality--statements which one can in turn verify in observation--then there is something wrong with the empirical generalization. And if one can deduce such true statements from an empirical generalization, this constitutes evidence for the truth both of the empirical generalization and of the statements deduced from it.

(c) Deduction as a Method of Discovery

Whewell claims that any conclusions arrived at by means of deduction are "virtually" included in certain premises. I doubt whether this is an accurate description of the deductive process. Deduction seems to me a process of proceeding by rule, rather then a process of "unfolding." Furthermore, I think that Whewell sometimes employs a process which he calls deduction, but which it is very difficult to represent as an "unfolding." This particular employment of deduction appears in two different contexts.

(1) Whewell asserts that the formal sciences are grounded in axioms and definitions. But in showing how a science is deduced from these, it is my opinion that Whewell does not, in all cases, "unfold" axioms and definitions. Instead of unfolding them he uses them as rules. Or he appeals to them as proof of a conclusion, not because some particular conclusion has been unfolded out of them, but because the conclusion has the same form as some axiom. The axiom is true. Therefore, all statements of that form are true. The conclusion is true because the axiom is true. Its truth does not consist in the fact that the axiom "contains" it. I have already quoted a passage which illustrates this point: "Logic is a system of doctrine which lays down rules for determining in what cases pretended reasonings are and are not demonstrative."

(2) In the empirical sciences Whewell will not allow that deduction is employed as a method of discovery. Nevertheless it is, according to him, certainly employed as a method for predicting future events. And where the prediction is subsequently proven to be true, it seems to me quite proper to regard this as discovery. I grant that the original induction, which formed the basis of the subsequent deduction, was the "true" discovery. But when we predict on the basis of the induction, employing deduction to do so, I think we thereby discover new truths. We discover at least that certain cases which were not employed as data in arriving at the induction, can be deduced from the induction and, therefore, can be colligated by means of the same induction. And we

discover in the present, by means of calculation, what at least some of the characteristics of the universe are likely to be at a future date.

4. The Proofs to Which the Conclusions Arrived at by Deduction Are Amenable

Conclusions arrived at by means of deduction are ultimately amenable to only one kind of proof, namely, intuition. But the intuitions involved differ at least in this. Sometimes we resort to intuitions of particular "things" to verify particular conclusions; at other times we resort to intuitions of experience "as a whole" in order to verify some deductive system "as a whole."

(a) The Intuition of Particular Things

Where the intuition of particular "things" is concerned, (1) some are intuitions of formal properties, either as properties of experience, or in and of themselves. And (2) some are intuitions of objects. The distinction is approximately that between "inner" and "outer" intuitions. But it is not quite this because frequently "inner" intuitions, for example, the intuitions of the Conceptions derivative from the Idea of space, have "outer" counterparts.

At any rate, what is needed to prove all the deductions which may be performed are (1) intuitions which will guarantee the truth of deductions present in the formal sciences, and (2) intuitions which will guarantee the truth of deductions present in the natural sciences. For example unless I can intuit what a Euclidean triangle is, I cannot follow the train

of deductions which culminate in the proof that the internal angles of a Euclidean triangle together equal 180°. Furthermore, unless I can see the moon on such and such a date I cannot verify my present deduction that there will be an eclipse of the moon on that date.

(b) The Intuition of a Deductive System "As a Whole"

A second approach to the validating of deductions is suggested by Whewell. Like the preceding method, it ultimately resorts to intuition. But unlike the preceding method, this second method employs an intuition of the system "as a whole." And what one intuits in such a case is that the system is logically consistent and that it "applies to" the empirical world taken "as a whole." Although some deductive systems do not apply to the empirical world in its entirety, they do apply entirely to one aspect of experience. For example, pure mechanics can be applied to all motions.

(c) Two Difficulties Arising from This View

Whewell becomes involved in a number of difficulties here, two of which I shall illustrate.

His first difficulty arises from the fact that he will permit _purely_ formal sciences, but not _merely_ formal sciences.[125] Whewell frequently asserts the possibility of purely formal sciences, built upon axioms and definitions and elaborated purely by the employment of deduction. He says, _e.g._,

> There is a _pure_ Science of Motion, which does not

> depend upon observed facts, but upon the Idea of motion. It may also be termed Pure Mechanism, in opposition to Mechanics Proper . . . which involves the mechanical conceptions of force and matter.[126]

But, for Whewell, such a science, while purely formal, is not merely formal, _i.e._, is not simply a self-contained set of tautologies. Such a science is for him not merely formal since motion, for example, has a necessary reference to space and time. Such a science has a subject matter, it is "about" something, namely, motion, space, and time. Again, such a science as "pure mechanism" would be highly mathematical in its form. The mathematics involved would be "pure" mathematics. But pure mathematics, although purely formal is, like pure mechanism, not merely formal. For pure mathematics also has a subject matter, namely, space and time.[127]

Such sciences, then, are not merely formal--_i.e._, "true" because tautological--but they are, nevertheless, purely formal. They can be constructed by referring only to the universal and necessary forms of motion, space, and time. And, although these forms are necessarily given in experience, the pure sciences of the forms are not empirical sciences. They are not about the forms as empirically given. They are, instead, solely about Ideas and Conceptions.

Since it is an historical fact that progress in the empirical sciences has had to wait upon progress in sciences like mechanics and mathematics we have here all the proof that can be demanded as to the possibility of pure sciences of mechanics and mathematics. But if it be allowed that pure

sciences--*i.e.*, sciences which are not dependent upon perception--are possible, a difficult question which arises is this. How can a conclusion arrived at deductively in such sciences be verified?

It cannot always be referred to specific intuitions, for in some cases, we have not yet had the relevant intuitions. Instead, beginning with other intuitions, we have employed the deductive method to arrive at a statement which, as yet at any rate, represents a "new" truth, provided it is true. The question is, Is it true? Whewell says that one thing we can do to verify such a statement is to go back over our calculations. If our axioms and definitions are seen to be proper statements concerning the relevant Ideas and Conceptions, and if we can "see" that we have made no errors in our deductions--*i.e.*, if each deductive step can be seen to possess "the clearness of intuitive evidence"[128]--then the conclusion is *necessarily* true.

But while Whewell formulates the above view on several occasions, when pushed to what seems to a modern reader a reasonable limit he will not stand by it. For example, he rejects non-Euclidean geometry. This, as I see it, indicates the first difficulty which Whewell encounters in this area. Although virtually obliged to support a coherence theory of truth where the pure sciences are concerned, he always ends up defending a correspondence theory. And this brings him face to face with his second difficulty.

Whewell's second difficulty is that he cannot employ the

correspondence theory, in its usual form, to support the pure sciences. The pure sciences are universal and necessary. And their correspondence with experience will not prove their universality and necessity. Experience may illustrate such sciences, but cannot prove their universality and necessity.

(d) Whewell's Solution of the Above Difficulties

Whewell's solution, here, is to modify the correspondence theory. He states at one point that a statement is true if the "facts" have the same relation to each other in the statement as they have in reality. This, I think, is an acceptable way to phrase the correspondence theory as it is generally understood. But for the reason given above, such a theory cannot be employed to establish the truth of all statements deductively arrived at. Whewell's solution is to say that such statements are true if they correspond, not to the world of objective fact, but to the formal properties which the knower contributes to the world of objective fact and in terms of which the objective world is determined.

But Whewell's attempted solution is not quite successful. I think that his rejection of non-Euclidean geometry proves that his attempt is not successful. If he had a satisfactory way of accounting for the truth of propositions of the formal sciences, then surely he would have accepted non-Euclidean geometry. I admit that I am simply appealing to authority here, since I am not a mathematician. But, at any rate, for whatever the appeal is worth, my view is that if Whitehead,

Russell, Einstein, and innumerable others found non-Euclidean geometries acceptable deductive systems, the probability of their being acceptable is very high. This being the case, it seems equally probable that there are flaws in Whewell's theory of truth relative to the purely formal sciences.

My view is that there are two major flaws in his theory. The first is that although he recognized certain statements as purely analytical,[129] he did not recognize that the truth of such statements might lie in their being tautological. The second flaw lies in his metatheory. Having set up Ideas and objects as antithetical elements, he permits each to determine the other to some extent:

> As perception of objects implies ideas[130] so, on the other hand, ideas cannot exist where sensation has not been. We cannot conceive space without boundaries or forms; now forms involve sensations . . . . We cannot say that objects create ideas, for to perceive objects we must already have ideas. But we may say that objects and the constant perception of objects have so far modified our ideas, that we cannot, even in thought, separate our ideas from the perception of objects.[131]

On this rock Whewell comes to grief. This can be shown very clearly, I think, by setting forth the arguments which he employs in rejecting Reid's non-Euclidean geometry.

Whewell tells us of "a very curious representation" made by Reid.[132] Reid imagined a nation, the Idomenians, who have no sense except that of sight. He claims that these people would have a notion of space much different from ours. Reid supposes the Idomenians to be surrounded by a sphere on which all visible appearances are presented. And he goes on to point out that to such persons any two straight lines which

crossed each other once would cross a second time, and that the sum of the angles of any triangle would be greater than two right angles. It is clear that such conclusions must derive from, or themselves be, axioms contradictory to the Euclidean axioms, and, presumably for this reason, Whewell refers to them as paradoxes and asks what account can be given of them. Whewell, on the basis of Berkeley's argument that the sense of sight alone could yield only a plane surface, rejects the possibility of perceiving such a space as that suggested by Reid and, in this way, brushes aside the paradoxes. We are not Idomenians, ergo no such space as that described by Reid can be perceived. And, therefore, there can be no axioms such as those which Reid suggests. Yet, obviously, Reid was able to conceive such a space. And it seems that Whewell might have treated this topic more seriously, had he not permitted his perception of space to legislate to his Idea and Conceptions of space. Since he perceived space as having Euclidean properties he could not conceive it as having any other properties.

5. The Limitations of Deduction

To the extent that deduction gives us the purely formal sciences it is independent of induction. But in this form the purely formal sciences are of no use, in a pragmatic sense, since they are lacking in empirical content. Moreover, there are no merely formal sciences, because the forms are always the forms of something, even though in such sciences

as geometry and arithmetic the forms can be treated independently of that of which they are the forms. Deduction unaided then, can yield logic and metaphysics and, perhaps, geometry and arithmetic, but it cannot yield the empirical sciences. Therefore, to the extent that deduction is a part of the empirical sciences its role is subservient to the inductive process.

> [The] process of drawing conclusions from our principles by rigorous and unimpeachable trains of demonstration, is termed <u>Deduction</u>. In its due place, it is a highly important part of every science; but it has no value when the fundamental principles, on which the whole of the demonstration rests, have not first been obtained by the induction of facts, so as to supply the sole materials of substantial truth.[133]

Whewell points out, but does not attempt very seriously to account for--except in terms of historical examples--the way in which the purely formal sciences--especially mathematics--and the empirical sciences, have progressed together.[134] This is considered by many philosophers to be a puzzle which, if solved, would yield great insight into a number of epistemological and ontological problems. But Whewell does not see any particular puzzle here. He sees the usefulness of mathematics as rooted in the fact that the objects of experience are necessarily spatial, temporal, and numerical. And they may also possess the characteristics of motion. Mathematics is simply an explication of conceptions derivative from the Ideas of space, number, time, and motion. And when experience exhibits these characteristics they, as exhibited in experience, originate in the same Ideas as do the conceptions of mathe-

matics. There is, therefore, nothing strange in the fact that pure mathematics should describe the empirical world. It must be emphasized, however, that to the extent that mathematics does describe the empirical world, it describes it only in part. The empirical world is more than, and something other than, mathematics.

There are, then, four major results which we may attribute to deduction. (a) It confirms induction. (b) Since, in many cases, progress in the deductive sciences must precede progress in the inductive ones, we may say that the deductive sciences directly contribute to future inductions. (c) Deduction gives us new truths in the sense that it gives us truths which we did not know before we performed the deduction. Admittedly these conclusions were, according to Whewell, "in" the premises from which we derived them. But we did not know, prior to performing the deduction, that they were "there." (d) The purely deductive sciences give us no statements which, in themselves, are what Whewell would call "important." Where important--*i.e.*, substantial--truth is concerned, deduction is the handmaiden of induction. Deduction proves what induction has "happily guessed." But the purely deductive sciences *per se*, are not to be regarded as of no interest. Deduction develops an ability to think clearly and to follow complex chains of reasoning. And this may be said to be one of its important results.

Whewell, I think, does not underrate the value of deduction. Both deduction and induction are essential to science.

But for reasons set forth above, perhaps we can say that induction is the more important of the two processes.

Whewell disagrees with Mill's estimate of the importance of deduction. Mill sees deduction as becoming the predominant type of science, whereas Whewell holds that induction--the introduction of a new element into our observations by means of an act of the intellect--is the most promising form for science to pursue.[125]

C. The Method of Intuition

1. The Difficulty of Explaining the Method of Intuition

I have very little to say about the method of intuition that I have not already said in discussing the data of intuition. There are two main points to stress. The first is that any person "minimally rational," as Descartes says, can resort to intuition. The second is that both induction and deduction can achieve the results which Whewell ascribes to them only if intuition be possible. There is, therefore, an intuitive method of knowing. But it cannot be explained except in terms of itself. The method of intuition is that method which resorts to intuition. The method can be illustrated by listing intuitions as examples. But this will illustrate the method only if the reader agrees that the examples listed are, in fact, intuitions. And this applies not only to individual intuitions, but to a system as a whole, and to every step in the system. If we take a deductive sys-

tem by way of example, unless it is self-evident that each step "follows from" the preceding there is no other way, according to Whewell, to prove that it does. On this point the method of intuition has to be differnntiated from a method which resorts to admittedly arbitrary rules and conventions.

2. The Employment of the Method of Intuition

The manner in which the method of intuition is to be employed in first order knowledge can only be set forth by stating instances in which it has been employed. Many such instances have been cited above. Intuition, and only intuition can, for example, be employed to prove that definitions, axioms, and inductions are valid. Here, of course, many writers disagree with Whewell.

One area in which few writers would disagree with Whewell is that the intuitive method is the only method which can be employed to discover the nature of intensive magnitudes. There would be less agreement as to whether or not the intuitive method is the only method which can be employed in using the various scales which have been designed to measure such magnitudes.

In first order knowledge there are two classes of "agents" involved in the employment of the method of intuition, namely, the senses and the dispositional powers of the mind. The senses alone could not provide intuitions, since sensation is non-cognitive. But without the senses we should be deprived of many of the intuitions which we now possess.

And, I take it, from what was said above concerning the relation of sensations to Ideas, that without the senses we might have no intuitions at all. But Whewell does not explicitly say that this is the case. It is clear that without sensations we should have no intuitions pertaining to objects. But we might still have intuitions pertaining to emotions, for example. And we might have intuitions pertaining to the dispositional powers of the mind, and to some Ideas and Conceptions. In other words, we might still have "inner" intuitions. Whewell does not make his position on this point clear.

In Whewell's philosophy of science--i.e., in second order knowledge--it is my view that there is only one class of agent involved, namely, the dispositional powers of the mind. Whewell's account of these powers is not very informative. He gives us some names--"sagacity," "invention," "genius," "reason," "intuition," and the like. But it is very difficult to find out from Whewell's writings what these names signify. One feature which they have in common is that they signify a power of the mind to know things immediately. If we call this power "intuition" then second order knowledge is arrived at primarily by the employment of intuition.

Philosophy of science, according to Whewell, provides two major kinds of knowledge. It provides knowledge of "the essence and conditions of all real knowledge." And it provides knowledge of "the best methods for the discovery of new truths." For knowledge of "the essence and conditions

of all real knowledge" we are dependent upon intuition, as I shall show in the following section where I discuss Whewell's theory and metatheory of knowledge. For knowledge of "the best methods for the discovery of new truths" we are ultimately dependent upon intuition. But, except in the case of the method of intuition, our knowledge of methods is not intuitive in quite the same way that our knowledge of the essence and conditions of knowledge is intuitive. Where our knowledge of the essence and conditions of knowledge is concerned we can resort to nothing but intuition. But our knowledge that deduction and induction are effective methods of knowing is supported by the universal consent of those who use them, and by the results which the employment of them achieves. Furthermore, the history of science makes clear the great advances in knowledge which occurred after these methods had been perfected and properly integrated. One of the best ways to convince ourselves of the effectiveness of these methods is to become practising scientists ourselves. The man who understands science also understands that deduction and induction are truly methods of knowing.

But our second order knowledge that deduction, induction, and intuition are the methods which yield first order knowledge appears to be primarily intuitive. A history of the inductive sciences, which provides us with considerable information on these matters, is inductive to the extent that it is empirical and historical. And it is deductive to what-

ever extent it employs syllogistic arguments. But if Kepler's discovery of the orbit of Mars is an instance of the employment of induction, and if the proof of a Euclidean theorem is an instance of deduction, then I, for one, cannot see that either Whewell's <u>History of the Inductive Sciences</u> or his <u>Philosophy of the Inductive Sciences</u> results from the employment of induction and/or deduction. Instead, in each of the above works, Whewell has presented certain data to the "sight which produces knowledge." In the former work he has presented a history of the employment of Ideas. In the latter he has presented the <u>rationale</u> of science itself. And the conclusions which he states about science have been arrived at immediately, not as a result of the employment of induction or deduction.

Second order knowledge, for Whewell, contains statements of two different kinds as was pointed out above. There are statements which express knowledge of the nature and conditions of knowledge, and statements which express knowledge of the methods of knowing. In addition to there being two different <u>kinds</u> of statements about first order knowledge in Whewell's writings, there are also, in my opinion, different <u>orders</u> of such statements. And, where these orders are concerned, I hold that there are two different classes of them. There are orders of descriptive statements. And there are orders of prescriptive statements.

As I stated in my introduction, if we call science first order knowledge, and mean by "first order knowledge" purely

descriptive statements about nature and experience, then science, for Whewell, is more than first order knowledge as so defined. For science contains universal and necessary statements. And he will not allow that these are merely descriptive. Furthermore, Whewell's philosophy of science also contains both descriptive, and universal and necessary, statements. It, contains, therefore, statements of a kind other than second order knowledge, if we mean by "second order knowledge" purely descriptive statements. Of course, we need not define "first order knowledge," "second order knowledge," and so on, as purely descriptive if we do not choose to do so. But (1) we need some kind of terminology to designate purely descriptive statements which stand in the relations indicated by "first order," "second order," and the like. And (2) C.D. Broad, from whom I have borrowed these terms, uses them as signifying purely descriptive statements. For the above two reasons, I propose to use them in this way also.

But a terminology is also needed to designate orders of prescriptive statements. There is no reason why these could not be designated "first order prescriptive statements," "second order prescriptive statements," and the like. But, these statements are characterized not only by being prescriptive. They are characterized also by the fact that they are more difficult to verify than are descriptive statements. For these two reasons I have elected to classify them under the terms "theory of knowledge," "metatheory of knowledge," "meta-metatheory of knowledge," and so on.

I have said, above, almost all that I wish to say about Whewell's account of purely descriptive statements. But I should like to investigate more fully how the method of intuition relates to Whewell's account of prescriptive statements. That is to say, I wish to see how the method of intuition relates to his theory of knowledge, his metatheory of knowledge, and so on.

3. Whewell's Theory, and Metatheories, of Knowledge

The first question I wish to answer is, What is Whewell's theory of knowledge and what justification does he offer for it?

Although it would take a good deal of exposition to set forth fully Whewell's theory of knowledge, I think it is unnecessary to do this. All that is vital to the theory can be stated in two sentences. (*a*) There are certain paradigms of knowledge, with Newtonian physics being the paradigm *par excellence.* (*b*) If we take Newtonian physics as the paradigm of knowledge then we can state a formula which is both descriptive of the sciences we now have and prescriptive of any science we shall ever have. Knowledge consists in the relating of particulars by means of activities of the knowing subject.

What verification does Whewell offer of this theory? If we cut through all the verbiage to the fundamental argument, it is that as a descriptive statement the "formula" stated in (*b*) above, is intuitively evident. Granted, it is not intuitively evident to everybody. But it is intuitively evident

to anyone who understands science. And since science is an example of "what we mean by knowledge" only those who are familiar with science are in a position to judge whether the formula is intuitively valid or not. But in order to justify the above formula as a prescriptive statement, a metatheory of knowledge is required.

Whewell devotes hundreds of pages to the elaboration of what I call his metatheory. In all this material I find three fundamental assertions. (a) There are particulars. In other words, first order knowledge, which deals with particulars, is wholly genuine. (b) Particulars can be classified by the employment of dichotomous schemata. The two most general classes of particulars are given a variety of names by Whewell. He speaks of subjective particulars—objective particulars, inner particulars—outer particulars, Ideal particulars—sensible particulars. The most significant feature of the members of these most general classes is that the members of the one and the members of the other are antithetical. (c) There are media in which particulars are related.

Whewell's defence of the above metatheory is elaborate. But when all is said and done, the justification is that the above statements are, as descriptive statements, intuitively evident. In order to justify them as prescriptive statements, a meta-metatheory is required.

Whewell's meta-metatheory is not very extensive. Its most important statement is that there is a Medium--the Absolute (or, sometimes, God)--from which the particulars, and

the media which relate them, "flow" as from a "centre." This Medium, or Absolute, cannot be made an object of scientific inquiry.

As a descriptive statement, the assertion of the Absolute is intuitively evident to anyone who accepts as intuitively valid the preceding theory of knowledge statements and metatheory of knowledge statements. As a prescriptive statement--*i.e.*, that all knowledge necessarily has the quality of "flowing from" or "emanating from" an Absolute--it can only be justified by an appeal to a meta-meta-metatheory.

The meta-meta-metatheory, to the extent that I have been able to figure it out, contains the single statement that whatever is intuited as valid is valid. As a purely descriptive statement--*i.e.*, as a statement describing the intuitions previously noted--the statement, "Whatever is intuited as valid, is valid," is intuitively evident to anyone who accepts the preceding intuitions. As a prescriptive statement--*i.e.*, as a statement to the effect that whatever is known to be undeniably true must be so known through intuition--the statement would require a meta-meta-meta-metatheory to justify it. But I cannot find a meta-meta-meta-metatheory in Whewell's writings.

I should now like to make three points about the material in the above section, although they may be almost too obvious to merit their being set down. The above analysis is a structure which I have imposed upon Whewell's writings. It is not set forth in them. But, in my opinion, this structure is im-

plicit in his analysis. Moreover, every statement which Whewell puts forth as a true statement ultimately depends for its truth upon intuition. And finally, while it is possible to appeal to intuition on any level, there is no level on which we can finally justify the appeal. Instead, we are driven from metatheory to meta-metatheory, to meta-meta-metatheory, _ad infinitum._

CHAPTER V

THE GENERAL CHARACTER OF KNOWLEDGE ACCORDING TO MILL

In this chapter I wish to set forth briefly the items which Mill stresses most in his analysis of knowledge. These are as follows: 1. Inference. 2. The data of knowledge. 3. The grounds of the possibility of knowledge. 4. Logic as the science of science. 5. Realism. 6. The Import of Propositions. 7. The knowing situation. 8. The sciences. 9. Philosophy. 10. Certainty and the "whole of knowledge."

1. Inference

The analysis of knowledge, for Mill, is primarily an analysis of kinds of true statements and the processes which yield these statements. Of the various processes which yield true statements, inference is of most interest.

There are three kinds of inference. There is a kind of inference which I shall call precognitive. Mill discusses this at considerable length, but does not give it a name (other than "inference.") A second kind of inference is induction. Ratiocination is a third kind of inference. I shall now discuss these briefly. I shall also distinguish inference from (1) "merely verbal" statements and (2) from descriptions. It is also necessary to point out that inference, as an activity, has to be distinguished from an in-

ference, the result of the activity of the same name.

(a) Precognitive Inference

Precognitive inference, as an activity, yields (1) objects and (2) knowledge that something is an object. The knowledge which results from precognitive inference is awareness of things as objects.

Logic, Mill tells us, deals with whatever elements of our knowledge are not "original." And he will not allow that objects are original. He calls the original elements of "knowledge" feelings, and says that there are four classes of these--sensations, thoughts, emotions, and volitions.[136] These are the materials with which knowledge begins. Beginning with these materials, and employing an activity called inference, we go beyond the immediate evidence of consciousness and assert the existence of subjects and objects. That I am a subject, and not merely certain feelings, is an inference. And that an apple is an object, and not just certain colours and tastes, is an inference. This kind of inference is precognitive, since it is beyond recall. But it is a very important kind of inference, since it yields subjects and objects. Almost all the statements which science asserts as being true or false are about subjects and objects, not about feelings.

(b) Induction

"Induction" is a name both for a mental activity and for the results of the activity. In other words, it is necessary to distinguish between induction and inductions. There are

four characteristics of inductions which I wish to stress here. (1) Inductions are more general "truths" derived from less general "truths." Most of these truths are expressed as verbal statements, but they need not be. Animals, Mill claims, are capable of rudimentary inductions. The burned dog fears the fire as much as does the burned child. That some specific instance of fire burned--*i.e.*, seared the flesh and produced pain--is a particular statement. That all fire burns is an induction, because it is a general--in this case, a universal--statement. (2) Inductions "go from the known to the unknown." They assert, on the basis of past experience, statements which are held to be true of cases as yet untried. (3) Although inductions are of more than two kinds, there are two which are of particular interest. There are inductions which state "merely empirical" laws and "truly causal" laws. A law, in the present sense, states an observed uniformity in nature. A merely empirical law is one which holds only of those cases which we have examined. It is, therefore, properly only a description. But it is frequently very difficult to know whether or not a law is merely empirical or truly causal. As a result, some laws have to be held tentatively until we can be certain which kind of law they are. "All swans are white" turned out to be a merely empirical generalization. But we have reason to believe that "All men are mortal," and "The earth's rotation is the cause of day and night" are truly causal inductions. We have reason to believe that man and mortality, and the earth's rotation and day-night necessarily

"go together." But it turned out that whiteness and swans do not necessarily go together. (4) The rationale of inductions (and of ratiocinations) is summarized in the statement (itself an induction) that a mark of a mark is a mark (nota notae est nota). This orientation replaces the rationale offered by the schoolmen to the effect that induction involves the intuiting of universals.

(c) Ratiocinations

Here, again, it is necessary to distinguish ratiocination, as an activity of the mind, from ratiocinations, i.e., conclusions reached by ratiocination. And, again, it is the conclusions, not the activity, in which I am interested at the moment.

As a result of that activity called ratiocination we begin with statements of a certain degree of generality and reach conclusions of an equal, or of a lesser, degree of generality, than those with which we begin. A ratiocination is thereby distinguished from an induction. For example, Kepler's law (which, for Mill, is a description, not an induction) asserts that the planets move in elliptical orbits. This law can be deduced from Newton's theory. Newton's theory holds that the motion of the planets is to be accounted for in terms of the composition of a centripetal force towards the sun and a projectile force away from it. Because of the composition of these two forces, therefore, the planets move in elliptical orbits. The above "deduction" is an example of

what Mill means by ratiocination.

(d) Inference Versus Merely Verbal Statements

An inference is a "new" truth. It is a genuine addition to knowledge. But "merely verbal" statements are not new truths. Some merely verbal statements are true simply on the basis of convention. Others are but alternate ways of asserting something already known. "'Man' means Socrates, the Duke of Wellington, and so on," is an example of the first type of purely verbal statement. It is not, strictly speaking, capable of being either true or false. It is only capable of conforming, or not conforming, to usage or convention. " All A is B, therefore some A is B," "Socrates is a man, therefore Socrates is a living creature," are examples of the second kind of merely verbal statement. In both these cases the conclusions set forth no new truth. What is asserted in the conclusions is already apparent to anyone who understands the antecedent assertions.

The major use to which Mill puts the distinction between inference and merely verbal statements is to elaborate and substantiate his contention that no reasoning takes place within the traditional forms of the syllogism. The syllogism simply puts forth, in a clear and formal way, something we have already learned. The reasoning which results in the conclusion set forth in the syllogism actually precedes the employment of the syllogism.

(e) Inference Versus Description

Description, like merely verbal statements, does not add to our knowledge. Description simply records something which we already know. Verbally, many descriptions have the form of inductions. Descriptions are frequently general, and sometimes universal, statements. But they differ from inductions in that an induction goes from the known to the unknown, whereas a description does not. "All the Apostles were Jews" is a description in spite of its general form. Similarly, "All the planets move in ellipses" is a description. But, "All planets move in ellipses" is an induction.

As has already been noted, the difference between an induction and a description, and also the precise nature of an induction, were two of the main points of controversy between Whewell and Mill.

2. The Data of Knowledge

(a) The Data of Induction

The data of induction for Mill are phenomena, abstractions from phenomena, and a number of intuitively discernible relations among phenomena. If we mean by phenomena the things which can be named, then phenomena may be subdivided into feelings, substances, and attributes. Feelings are of four kinds, as noted above. Substances are of two kinds--subjects and objects. And attributes are of three kinds--qualities, quantities, and relations.[137]

Mill also offers the following classification of "all

Nameable Things." This is intended to be the "same" classification as the foregoing, except for certain changes in terminology, and it yields four classes of nameable things: feelings, or states of consciousness; the minds which experience those feelings; the bodies, or external objects, which excite certain of those feelings, together with the powers or properties whereby they excite them, "these last being included rather in compliance with common opinion . . . than because the recognition of such powers or properties as real existences appears to be warranted"; and the succession and coexistences, the likenesses and unlikenesses, between feelings or states of consciousness.[138]

By means of a thought process called abstraction I am able to attend to a single feature of a phenomenon. I thereby provide myself with the data of the "purely formal sciences," as Whewell would call them. For example, by attending only to the self-identity of a phenomenon I provide myself with the arithmetical datum, "1." By attending only to the length of a phenomenon--e.g., a board--I provide myself with the geometrical datum called a straight line. And so on.

(b) The Data of Deduction

Mill has two meanings for deduction, since there are, for him, two kinds of deduction. And each kind has its own data.

One type of deduction is formalized in the syllogism. The other type is equivalent to the employment of what Mill calls the concrete deductive method. The premises of the

syllogism are inductions which state relations other than causation. "All men are mortal" provides an example. The data of the concrete deductive method are laws of nature found out by induction.

(c) The Data of Intuition

It is my opinion that Mill does not succeed in stating unambiguously that element of knowledge which either is, or can be, intuitively known. He offers us no definitive statement on this matter. He does hold that some of the knowledge we possess is known prior to inference. Such knowledge he calls "primitive data."[139] Such data are the subject of intuition or consciousness.

He uses these two terms interchangeably. But often he seems to mean by intuitive data that which is perceived as here-now. And much of what is perceived as here-now is perceived as the result of inference, not prior to inference. I perceive an apple as an object. And, as far as conscious awareness is concerned, this is an intuitive perception. That the apple *qua* object is so perceived as a result of inference, is at best a theory. If we hold, as most commentators do, Mill's view to be that the only intuitive data are pre-inferential, then we shall have to say that the data of intuition for Mill are primitive states of consciousness--emotions, volitions, and sensations. But there are some serious difficulties associated with such a view especially where sensations are concerned. I shall elaborate this problem later.[140]

3. The Grounds of the Possibility of Knowledge

I shall not attempt to set forth all the grounds of the possibility of knowledge in Mill's case, but only those which he examines in greatest detail. For Mill knowledge is dependent upon (a) the orderliness of nature and (b) certain operations which we can perform and which we can show to be grounded in the "nature of things."

(a) The Orderliness of Nature

That nature is orderly, Mill does not doubt. To be strictly accurate, one would need to say that knowledge, for Mill, exhibits the orderliness of experience, rather than of nature. But Mill really does not make much of an issue of this distinction.

The phrase "the orderliness of nature," in Mill's case, summarizes several conclusions, all of these conclusions being inductions from experience. Nature exhibits many recurrent patterns. There are, for example, temporal series, causal series, spatial configurations, patterns of relation. Not only does nature exhibit these patterns now. Nature will continue to exhibit them. The future will resemble the past. The above kinds of orderliness may also be called kinds of permanence. Events come and go, but the patterns they exhibit are repeated. Not only is there permanence in the pattern of events. There are also things which are permanent in the sense that they endure. A major function of these enduring things is that they constitute "permanent causes."

Mill's usual approach to cause is in terms of temporal series. If event A "unconditionally" precedes event B, then A is the cause of B. But then we may seek the cause of A, and the cause of the cause of A. And so on. This process does not go on to infinity. Eventually we come to items of which we cannot discover the cause. Such items Mill calls "permanent causes." An example of a permanent cause is the earth's rotation. There are, therefore, for Mill, two kinds of permanence in nature-- recurrent patterns, and enduring things. I wish to discuss these briefly and will begin with the recurrent series which nature exhibits.

The orderliness of events is of two kinds--orderliness of succession and orderliness of coexistence. In this paragraph I wish to outline Mill's views about orderliness of coexistence. Orderliness of coexistence refers to all properties of things not dependent on time. Algebra, for example, deals with that regularity which we call equality, arithmetic with that regularity possessed by things in virtue of their being numerable, geometry with that regularity possessed by all those things which are extended. These three are the sciences frequently offered as paradigms of knowledge, usually because of the certainty which their conclusions exhibit. But, while Mill grants that their certainty is desirable, he does not recommend that we take these as models which all other sciences must imitate. A peculiarity of the mathematical sciences is that their subject matter is abstract, that is to say, it is arrived at by the mind's focussing its atten-

tion on only a part of the data before it. Algebra, for example, deals only with the equality of equal things; the things themselves are left out of account. But in many departments of knowledge it is the thing in its concrete wholeness which interests us. Algebra may be entirely true, whereas many of the hypotheses of psychology may be extremely tenuous, but since algebra contains the minimum of empirical content--the relations between equals--it also exhibits the minimum of significance. To the extent that it can be employed in furthering our understanding of nature it is to that extent significant. In and of itself it is of little interest. And even though we desire in all branches of knowledge that certainty which algebra exhibits, it is the certainty, not algebra as a body of knowledge, which recommends it to our attention. The same is true of the other branches of mathematics. We are interested in them when we can employ them to further our knowledge of nature. But we do not regard them as paradigms of knowledge. Rather they are elements within knowledge. It is desirable that the other kinds of knowledge be as certain as mathematics. But this certainty cannot be introduced simply by employing the mathematical method. The mathematical method cannot be the method of the other kinds of knowledge because the subject matter of the other kinds of knowledge is not amenable to such a method. Nor will introducing mathematics into the other kinds of knowledge secure for them the certainty of mathematics. It will, of course, secure this end to the extent that mathematical data

are a part of the data to be dealt with. But to the extent that the data are other than mathematical, some method other than a mathematical one must be found. And, for the most part, the "concrete deductive" method is the method to be chosen.

Mill's account of orderliness of succession is usually expressed in terms of the temporal patterns exhibited by our conscious states. But in providing the <u>rationale</u> of our knowledge of succession he finds it necessary to posit what he calls by the synonymous expressions "original natural agents" and "permanent causes." One reason why Mill asserts "permanent causes" is because he is unable to regard certain questions as insignificant and meaningless. I have in mind such questions as: Why is there motion? Why is there change? Why is there consciousness? He frequently tells us that we cannot hope to secure answers to these questions (except from the metaphysicians, for whom he often seems to have little respect[141]). Yet he cannot avoid asking such questions, either. Mill's approach here is to say that the questions are meaningful, and therefore theoretically, there must be answers to them. The answers would be in terms of causes, in the sense of forces--"unknown" forces which we infer from "their" effects. But, while we see the necessity to assert the reality of such causes, we can have no immediate knowledge of these causes. We can know that there are such causes. But they are not objects of possible experience. We always experience only their effects--or what we assert to

be their effects. Permanent causes differ from other causes also in that we can often discover the causes of causes; but we cannot discover the causes of permanent causes.

At first glance there seems to be a radical and fundamental difference between Mill and Whewell in their views concerning our knowledge of the orderliness of nature. Whewell maintains for a good many hundreds of pages, that we, as knowers, introduce the regularity. Mill is equally insistent that we discover the regularity. But their inquiries have this, at least, in common. As their inquiries proceed they both see a necessity to assert an order which exists prior to our knowledge of it. Whewell ascribes this order to the Ideas of the Creator. Mill ascribes it to what he calls "permanent causes" or "original natural agents.[142] For Whewell the Ideas of the creator are unknowable in the sense that they cannot be completely known. For Mill the permanent causes are unknowable in the sense that we cannot state their causes. When we reach ultimate facts we discover that they exhibit an inexplicability. At this level "the real stumbling block is perhaps not in any theory of the fact, but in the fact itself."[143] The Divine Ideas, asserted by Whewell, and the permanent causes asserted by Mill, perform essentially the same function, that of explanation. But as ultimate terms of explanation they themselves cannot be explained. Yet, interestingly enough, they do not for this reason make knowledge essentially irrational. Whewell contends that they do not lead to

irrationality because they are the product of a divine reason. Mill contends that it is reasonable to accept some propositions as true, without asking for proof of their truth.

> What are the propositions which may reasonably be received without proof? That there must be some such propositions all are agreed, since there cannot be an infinite series of proof, a chain suspended from nothing.[144]

But on what grounds can it be asserted that some of these propositions which we may reasonably accept without proof are about agents which are known to be "original," and about causes which are known to be "permanent"? The answer to this question has to be couched in epistemological terms, I think. It is not that we know them to be final, even though we do know that they represent the present limits of knowledge. Rather the situation for Mill is as follows. The only way to go beyond original natural agents would be to assert noumena, to assert that something not only unknown, but unknowable, is the ultimate ground of knowledge; and this would be only an escape from bad to worse. It is better to say that our knowledge has known limits, than to say that its grounds are unknowable. The importance, in fact the necessity, of stating these limits is derivative from an orientation to knowledge which holds, with Aristotle, that without a first cause there is no cause at all. Now, obviously, in one sense there can be causes for Mill without there being a first cause. A cause can be determined wherever an unconditional series of events is discovered. Why then posit original natural agents and permanent causes? There seems only one explanation, a feeling on Mill's part that he

was obligated to offer a reason for the orderliness of nature, a reason expressed in terms of something constant and permanent, something other than the orderliness of nature itself.[145] A major function of this "something permanent" is that we can know the changing by relating it to the unchanging. And the knowledge which we thereby attain is not the same as that which we arrive at by means of induction or deduction. The knowledge arrived at by science is knowledge of nature. That there "must be" something permanent is primarily something which we know about knowledge, namely that knowledge must be grounded in permanency.

(b) The Operations Which Result in Knowledge

Knowledge, for Mill, is the result of many operations. All that I wish to do here is list the most important of them, and to make a few remarks about three of them. The most important of the knowing operations are observation, especially when enhanced by the employment of the Four Methods, abstraction, naming, classification, intuition, induction, and deduction. The last three items, above, are what I have called the methods of knowing. It is clear that it is arbitrary to single out these three and call them *the* methods of knowing since observation, abstraction, naming, and classification are also methods which we employ in order to arrive at knowledge. But Mill calls such operations as observation, abstraction, *et al*, operations "subsidiary to" *the* knowing operations.

There are two points which I wish to make about Mill's discussion of *the* methods of knowing--*i.e.*, intuition, induc-

tion, and deduction. (1) I wish to repeat, since it seems to me very important, a point which I have already discussed. The point is that not all knowledge can be inferential. If there is to be inferential knowledge there must be something known prior to inference. Mill certainly holds that we have knowledge prior to inference.[146] Such knowledge he calls intuitive. If there is intuitive knowledge, then there must be a method of arriving at it. I have called this "method" the method of intuition. Mill actually says very little about such a method, although he does say a great deal about intuition and intuitive knowledge. (2) By combining the methods of knowing in various ways, Mill outlines a number of additional methods. These are the chemical, or experimental method, the geometrical, or abstract, method, the physical, or concrete deductive method, and the inverse deductive, or historical method. Mention must also be made of what are universally referred to as "Mill's Methods."

4. Logic as the Science of Science

Logic is the science of the operations of the human understanding in its pursuit of truth. As a science, it is the science of the method of science. Logic makes explicit the rationale of scientific inquiry. "The logic of Science is the universal logic, applicable to all inquiries in which man can engage."[147]

The major concern of logic is proof. Logic provides the forms, and the rationale, of proof. It does not provide proofs,

but teaches what constitutes proof. It does not point out which particular facts prove some other fact, but, instead, points out the conditions to which all facts must conform if they are to qualify as proof. To decide whether any particular facts fulfil these conditions in any particular case, belongs to whatever science deals with that kind of fact, not to logic.

Mill says that logic is both a science and an art. As a science it consists in the analysis of the mental process which takes place whenever we reason. As an art it consists in the rules, grounded on the above analysis, for conducting the process correctly. Logic is not the science of thinking _per se_. It is, instead, the science of valid thinking. Neither is logic the science of belief. Instead it is, as already noted, the science of proof, or evidence. Two claims, implicit in the above material, should also be made explicit. These are (1) logic is independent of psychology, and (2) logic is independent of metaphysics. These two claims are discussed at some length in Chapters VI and VII, below.

5. Realism

Mill, like Whewell, is an epistemological realist. The essential difference between Mill and Whewell on this point is that it is very hard to be certain whether or not, for Mill, the reality of objects _per se_ is _immediately_ given to conscious awareness. Mill has no doubt that objects are given to consciousness. But whether they are immediately given is a problem.

A question which exists in my mind on this point is as follows. When Mill argues that objects are the result of inferences of which I am not aware, does he mean that I performed the inferences so long ago that I have now forgotten them? Or does he mean that I am continually making <u>unconscious</u> inferences of the sort that yield objects (and subjects)? In some of his references to Berkeley's <u>New Theory of Vision</u> it would seem that the former might be the case. There was, for example, a time when I did not perceive a three dimensional space. But I perceive a three dimensional space <u>now</u>. And, similarly, I perceive three dimensional objects now. My perception of a three dimensional space now can be understood in terms of a history of my perceptual development. But when he speaks of inferring my father from the presence to consciousness of certain colours, it seems that I am continually inferring objects from sensations. At any rate, most commentators hold that it is Mill's considered and final opinion that objects are not immediately given as such to consciousness. It is also certain that Mill asserts on numerous occasions that knowledge is, for the most part, knowledge of objects.

An unobjectionable way to state Mill's realism is to state it in terms of "real" propositions. There are real propositions, which predicate truly, affirmatively or negatively, of one or more objects, existence, or order in space, or order in time, or causation, or resemblance.

6. The Import of Propositions

In this section I wish to do two things. First, I wish to outline Mill's views concerning what he calls "the import of propositions." This I shall discuss under the heading "The Primary Matters of Fact." Secondly, I wish to examine Mill's use of "existence" and "exists."

(a) The Primary Matters of Fact

Mill begins his chapter, "Of the Import of Propositions" by pointing out that such an inquiry could analyze either the state of mind called belief, or analyze what is believed. It is the latter which is of interest to him, he says, not the former, since what is of primary importance to the logician is the relation between the phenomena of which the subject and predicate terms are names:

> We have to inquire, then, on the present occasion, not into Judgment, but judgments; not into the act of believing, but into the thing believed. What is the immediate object of belief in a Proposition? What is the <u>matter of fact</u> signified by it? What is it, to which, when I assert the proposition, I give my assent, and call upon others to give theirs? What is that which is expressed by a form of discourse called a Proposition, and the conformity of which to fact constitutes the truth of the proposition?[148]

In defence of the correspondence theory of truth suggested in the above quotation, Mill asserts that the truth of a proposition does not derive from the inclusion of one class of names within another. If "All men are mortal" is a true statement, it is true not because the class <u>man</u> is wholly included in the class <u>mortal</u>, but because the attributes con-

noted by "man" are always accompanied by the attribute connoted by "mortal." The truth of "All men are mortal," therefore, "is not a question of the signification of names, but of laws of nature."[149] "We place the individual in the class because the proposition is true; the proposition is not true because the object is placed in the class."[150]

The truth of a proposition, then, being determined by "conformity to fact," it now remains to point out the kinds of assertions whose conformity to fact we can investigate. Mill decides that there are five such kinds of assertion:

> Existence, Coexistence, Sequence, Causation, Resemblance: one or the other of these is asserted (or denied) in every proposition which is not merely verbal. This five-fold division is an exhaustive classification of matters-of-fact; of all things that can be believed, or tendered for belief; of all questions that can be propounded, and all answers that can be returned to them.[151]

The assertions made in the above paragraph apply, of course, only to "real" propositions, not to "merely verbal" ones.[152] In the case of real propositions "the import of the copula is constant accompaniment of attributes . . . . In this way the object of belief in a proposition becomes 'either the coexistence or the sequence of two phenomena;' or one of the other primary matters of fact."[153]

I think it is fairly clear why coexistence, sequence, causation, and resemblance are included among the primary matters of fact. Coexistence and sequence (or succession) are principles of association. Causation is a particular form of succession. Resemblance performs many functions.

It is appealed to to explain the abstraction of single attributes, and the formation of the "bundles" of attributes which names have as connotations. It operates in generalization, and in the processes of comparison and contrast referred to in Mill's discussion of the Four Methods. It is basic to the "operations subsidiary to induction," to analogy, and to the precept <u>nota notae est nota</u>.[154] But the presence of existence among the kinds of primary matters of fact is more difficult to understand. I wish now to examine Mill's use of "existence" and "exists" in some detail.

(b) <u>"Existence" and "Exists"</u>

There are two questions which I wish to investigate here. (i) For Mill, is "existence" the name of anything? (ii) For Mill, does "exists" have only one, or more than one meaning? I shall discuss these in order.

I think it is clear that Mill held "existence" to be the name of something. Mill does not explicitly state that "existence" is a name. Nevertheless, it is my opinion that he does consider that it can be a name. I will now outline my reasons for thinking that this is Mill's view.

The first item to which I wish to call attention is that Mill does not mention "existence" in his discussion of syncategorematic terms. This, however, does not prove that Mill does not regard "existence" as a syncategorematic term, since he does not offer us an all-inclusive list of such terms. But since he defines a categorematic term as "a word which could

be used either as the subject or predicate of a proposition"[155] and since "existence" can be so used, as I show in the following paragraph, I think we must say that Mill held "existence" to be a categorematic term. It is also clear that categorematic terms are names for Mill.[156] If, therefore, "existence" is a categorematic term, it names something.

Secondly, in his discussion of the verb "to be" Mill claims that it has two possible employments. Sometimes it is "a mere sign of predication."[157] But at other times it signifies "to exist."[158] "The employment of it as a copula does not necessarily include the affirmation of existence."[159] But sometimes it does.[160] Furthermore, "the copula has a meaning of its own, in virtue of which it may itself be made the predicate of a proposition."[161] Since the predicate of a proposition is always a name for Mill, the copula, which as a predicate may be designated either "being" or "existence" is, it seems, sometimes a name.

Thirdly, as Mill points out in discussing the import of propositions

> in all propositions of which the predicate is a concrete name, what is really predicated is one of existence, coexistence, causation, sequence, or resemblance, and when a proposition consists of a subject and predicate which are abstract terms, it consists of terms which must necessarily signify one or other of these [five] things. When we predicate of anything an abstract name, we affirm of the thing that it is one or other of these five things.[162]

I interpret the above passage to mean, in part, that some subject and predicate terms name existence. If so, existence is something. It must be acknowledged, however, that a

statement such as "some abstract term signifies existence," could be interpreted as asserting only that some thing--*e.g.*, whiteness--exists. Or, to take another example, a statement such as "Whiteness is an existence" may only assert that whiteness is an identifiable nameable, thing.

Such an interpretation would be consistent with what is said in Book I, Chapter 3, Section (ii), of the *Logic* where Mill discusses "the most general names." Here Mill warns against the fallacy of supposing that all names are names of substances. Having issued the warning, he then attempts, without success, to seek a most general name which can be the name of any *thing*. He considers, and rejects, as a most general name, "object," "thing," "being," and "entity." He rejects all of these because they have become "tainted" "by custom." Because of their customary employment, such names have come to be considered names of substances. Mill, however, wants a term which will signify not only substances, but also other kinds of things, such as attributes and feelings. It is possible, therefore, that existence is, for Mill, among those *things* which are not substances. On this interpretation "existence" would remain the name of something, without being the name of a substance.

Fourthly, in one passage Mill speaks of existence as "the power of producing phenomena."[163] Presumably, then, "existence" names this power.

Fifthly, I interpret some of Mill's critics to be saying that Mill considers "existence" to be a name. For example,

in the following passage Stebbing seems to me to imply that, for Mill, "existence" is a name in the sense that it both connotes and denotes something:

> The recognition that descriptions may apply to nothing throws light upon a problem that has perplexed philosophers and has led to the introduction of the doctrine that there are different universes of discourse, in the sense in which 'different universes of discourse' means 'different modes of being.' Mill's view, for example, that every connotative word has denotation implies this doctrine although he does not explicitly develop the consequences of it.[164]

In "the Theory of Meaning" Ryle states that "Mill . . . takes it for granted that all words, or nearly all words, are names."[165] I think that the qualification intended by the phrase "or nearly all words" in the above quotation from Ryle, refers to the syncategorematic terms which Mill explicitly recognizes. Ryle also says, "Most words and descriptive phrases, according to . . . [Mill], do two things at once. They denote the things or persons that they are, as he unhappily puts it, all the names of. But they also connote or signify the simple or complex attributes by possessing which the thing or person denoted is fitted by the description."[166] I am, therefore, of the opinion that Ryle would say that, for Mill, existence is the name of something. Ryle does not explicitly say that Mill commits the "'Fido'-Fido" fallacy where "existence" is concerned. But I take it that Ryle thinks that Mill does commit it.[167]

I shall summarize the above discussion as follows. It is my view that Mill considers "existence" to be the name of

something. I hold the view for the following reasons. (i) Mill does not list "existence" as a syncategorematic term. (ii) "Existence" and "being" are, in some instances, synonymous terms. "Being" can be made the predicate of a proposition. Therefore, it is possible that "existence" can also be the predicate of a proposition. (iii) When we predicate a term of something, one of the things which such predication is capable of affirming is that the thing, of which we have predicated some term, is existence. (iv) "Existence" is the name of existence--i.e., of the power of producing phenomena. (v) My reading of Mill's commentators leads me to believe that they hold that "existence" is, for Mill, the name of something, since it denotes something.

I wish now to leave the question, "Is 'existence' the name of anything for Mill?" and take up the question, "What meaning does 'exists' have for Mill?" Where the answer to the latter question is concerned, I find that the critics disagree. Morton White says, "There is a view that may certainly be attributed to John Stuart Mill, according to which the word 'exists' is univocal."[168] Windelband says:

> It was John Stuart Mill who first brought his countrymen back to Hume's conception of associational psychology. Without asking what matter and mind are in themselves, the student should proceed from the fact that the corporeal and mental states form two domains of experience, completely incapable of comparison.[169]

I think it is correct to extrapolate Windelband's view as follows. The word "exist" has different meanings in "There exist mental states" and "There exist corporeal states."[170]

It seems, therefore, that when I state what I think Mill says about the meaning of "exists" I shall necessarily expound a view which someone else says is incorrect.

My view is that Mill uses the term "exists" equivocally or, as White would say, multivocally. I do not propose to determine all the meanings which "exists" has for Mill. I merely wish to show that it has more than one. This I shall now proceed to do. But first I want to say that the meanings illustrated in each of the following subsections sometimes overlap the meanings illustrated in the others.

(i) Mill notes that there are a great many statements which are meaningful but which are about such things as centaurs, ghosts, and the like. Now, a significant proposition implies the existence of a subject, because "in the case of a non-existent subject there is nothing for the proposition to assert."[171] Mill takes as an example, "The ghost of a murdered person haunts the couch of the murderer."[172] If "'the ghost of the murdered person' has no denotation, then the proposition is about nothing; but it clearly is not a meaningless noise; hence it must be about something, and what could it be about except ghosts?"[173] "Ghosts exist" is, therefore, a meaningful assertion. When we say "Men exist" we mean that men really exist (or have real existence.[174]) But we may decide that ghosts, like centaurs, "are a fiction of poets."[175] I think, therefore, that there are two meanings of "exist" here--one an assertion of real existence, the other of fictional existence. When we say that ghosts and

centaurs exist we mean, provided we reject the real existence of ghosts and centaurs, that they are referred to in works of fiction. When we say that men exist, we mean that men are objects of possible experience.

(ii) I also hold that, for Mill, the meaning of "exists" is different in the sentences "Objects exist," and "Subjects exist." To say that an object exists is to say that an object is the cause of sensations. To say that a subject exists is to say that a subject is a percipient of sensations. It would be incorrect to say that "an object exists" means that an object perceives sensations. I think, therefore, that there are two meanings of "exist" involved here also.

(iii) Another distinction which Mill recognizes, and which I have already discussed, is that sometimes "exists" merely signifies predication. But at other times it asserts "real" existence.

(iv) I believe that the following sentences also exhibit different meanings of "exist": "Substances exist," and, "Sensations exist." "Substances exist" means that substances are <u>known</u> as permanent possibilities of sensation. "Sensations exist" means that sensations are <u>felt</u> as here-now. "Substances exist" might also be said to mean, "Some experience has the quality of perdurability." By way of contrast to this we may say that "Sensations exist" means that some experience lacks the quality of perdurability. The distinction here may also be brought out in the following way. "Substances--<u>i.e.</u>, permanent possibilities of sensation--exist" means "Substances

are common to us and to our fellow creatures."[176] "Sensations exist" means that sensations are private. Or we may say that "Substances exist" means that substances are independent of our perception of them. By a substance

> we mean something which exists when we are not thinking of it; which existed before we had ever thought of it, and would exist if we were annihilated; and further that there exist things . . . which never have been perceived by men."[177]

"Sensations exist," on the other hand, means that sensations are being felt.

(v) Finally, I shall point to an ambiguity in the following sentences: "The sensations which I call a toothache exist," and, "Matter exists." "The sensations which I call a toothache exist" means, in one specific context, that a toothache is the _known_ cause--in the sense of invariable antecedent--of my going to the dentist. "Matter exists" means "matter is the unknown cause of sensations."[178]

7. The Knowing Situation

The model of the knowing situation for Mill, is what is generally called the "spectator" model. According to this model, to be known is to stand in certain relations to a knower. Of these relations, one may be said to be a relation _sine qua non_: to be known is to be present to the conscious awareness of a conscious being. There is some doubt as to whether Mill always supports the spectator model. This is especially true where knowledge of our conscious states is concerned. But my interpretation is that even these are

known to an "I," or a subject, which for epistemological purposes at least, is something other than the known states. He points out that all we are actually aware of is a certain "thread of consciousness." Yet he holds, nevertheless, that there is

> a something which I call Myself, or, by another expression, my mind, which I consider as distinct from these sensations, thoughts, etc., a something which I conceive to be not the thoughts, but the being that has the thoughts, and which I can conceive as existing forever in a state of quiescence, without any thoughts at all.[179]

However, such a self is for the most part, for him, of interest to ontology or to psychology but not to epistemology.

According to the way in which Mill interprets what I have called the spectator model, man is the observer of nature and is not, as Whewell argues, the interpreter of nature. The knowable is "there" to be known. It is necessary to point out that it is I, not Mill, who claims that Mill adopts the spectator model of the knowing situation. I do not find any doubt in my mind as to how Mill viewed the situation in which other knowers find themselves. For example, in his references to the discoveries made by Kepler, Newton, and other scientists, Mill argues (frequently against Whewell) that these men merely described what they observed. It is more difficult to find out how Mill viewed the knowing situation in which he found himself when writing his Logic. I argue, however, that the spectator model holds here also. I discuss this problem at some length in the preamble to my concluding chapter.

8. The Sciences, or, the Kinds of Knowledge

In order to present Mill's general orientation to knowledge I think it is necessary to say something about what he conceived the kinds of knowledge to be.

Mill discusses the kinds of knowledge from a variety of points of view. I wish to deal with three of them. I shall discuss this matter in some detail in the present section, since I do not plan to return to it.

The broadest classification of the kinds of knowledge found in Mill divides them into knowledge of data, knowledge of method, and knowledge of science--*i.e.*, knowledge of that which results from the employment of certain methods upon certain data.

A second classification divides knowledge into knowledge of particulars--"Particulars alone are capable of being subjected to observation"[180]--knowledge of coexistence, and knowledge of succession.

All knowledge beyond direct perception consists in discovering the manner in which the particulars given in perception are related. Algebra, for example, extends the perceived relation of equality, working out in great detail the implications of the empirically given truth that if equals are added to, or subtracted from, equals the results are equal. This science begins with a variety of phenomena which, among numerous other qualities, exhibit the relation of equality. Then the mind, or the thinking process, by means of abstraction takes up this characteristic of equality and elaborates it in-

to a deductive science. Syllogistic logic is similarly based. It rests upon what are called axioms, which are, in fact, generalizations from experience, and consists in the elaboration of these. Geometry differs from the other branches of mathematics in that it has a spatial quality.[181] In "pure" geometry we examine images which we hold before our minds, or which are present in imagination. And in applied, or empirical geometry, we deal with the actually given objects of experience. In the empirical sciences such as astronomy, physics, psychology, and the like, a different problem faces us. The problem does not lie so much with the data since the data are reasonably obvious, except that we are in danger of being overwhelmed by the quantity of data. Instead the major problem is the relating of the data in a significant way. An obvious way to relate data is in terms of their contemporaneous spatial configurations. And in primitive sciences, such as astrology and phrenology, this was the manner in which the data were related.

But a better way, more significant both from a pragmatic point of view and also from the viewpoint of a proper insight into nature, has been found in many cases to be to relate the data in terms of their temporal configurations. It is not enough to point out which events at T^2 succeed the events at T^1. A true insight into temporal relations begins only when we recognize certain recurring sequences of events. When we observe that event^1 is *always* followed by event^2 we are on the threshold of scientific knowledge of temporal relations. Whe n

we are able to establish that event^1 <u>must</u> be followed by event^2 then we have arrived at knowledge proper concerning nature. This type of knowledge we call knowledge of causes, with event^1 being designated cause and event^2 effect.

Supporting such an orientation to knowledge Mill recognizes two major assertions which he claims are inductions from experience. The first is that in that portion of the universe with which we are familiar, every event has a cause. The second is that there is a constancy or uniformity about nature. If event^1 is the cause of event^2 today, it will also be tomorrow. Mill denies that we introduce the constancy into nature. The constancy is there and requires only to be discovered. What is known is objective for us as knowers even though the known is intimately related to our consciousness. Strictly speaking, what I know, *qua* data, is the content of my own consciousness. But most of the data are not known as conscious states. Even though I can, by a considerable effort of thought, "reduce" the data to the sensory and affective experience of the subject, knowledge goes far beyond such experience. Most of what I claim to know consists of inferences from the sensory and affective experience of the subject. For example, I experience certain subjective data, but I see a tree. And, except for psychology, the sciences deal with inferred entities, not with our sensory and affective experience. Astronomy, for example, does not deal with the relations between my sensations and/or between my ideas. Instead, it deals with the heavenly bodies. I do not say that "my idea of the sun is

the cause of my idea of day," but, rather that the sun is the cause of day:

> The notion that what is of primary importance to the logician in a proposition, is the relation between the two _ideas_ corresponding to the subject and predicate (instead of the relation between the two _phenomena_ which they respectively express,) seems to me one of the most fatal errors ever introduced into the philosophy of Logic.[182]

Mill's writings can also be made to support a general classification of the recognized sciences. In the material above, knowledge was simply divided into knowledge of particulars, knowledge of coexistence, and knowledge of succession. But different principles might have to be employed to classify the sciences _per se_.

Mill does not offer us a complete list of the sciences nor a classification of all the recognized sciences. But one can, at any rate, list the ones to which he refers. And one can suggest in very broad outline what he might accept as a classification of them. An attempt to classify the sciences is valuable because the principles which underlie such a classification reveal a great deal about a writer's orientation to knowledge. Such a classification indicates his criteria of knowledge, and also indicates his views concerning the present scope of knowledge. A classification of the sciences gives us a synoptic view of the "whole of knowledge," thereby giving us the general orientation towards knowledge which is held by the person who produces the classification. I do not propose to work out a classification of the sciences to which I think Mill is necessarily committed. Instead I

only wish to show the general character which such a classification might have.

A question which arises is, Would Mill support a genus-species classification or a serial classification? This is not an easy question to decide. Since Mill states that all knowledge is fundamentally inductive, this would provide one basis for a serial classification. He also states that deduction will be the method science will pursue in the future, and this suggests a different basis for a serial classification. However, I believe it is best, instead of classifying the sciences in terms of the degree to which they are either inductive or deductive, to classify them, in Mill's case, on the basis of whether they are fundamentally observational or experimental. Unfortunately, this does not give us two clearly defined groups, because Mill classes together those sciences in which experiment is impossible and those in which its scope is very limited. Astronomy is an instance of the first type, sociology of the second. Furthermore, with such a classification it is difficult to know what to do with mathematics and logic. At any rate, such a classification would rest on a genus-species arrangement.

One should also keep in mind Mill's sympathy for many of Comte's views, and it is possible that Mill would have accepted Comte's classification of the sciences without too much objection. It will be remembered that Comte subdivides science into mathematics, astronomy, physics, chemistry, biology, and sociology. Comte's classification is a serial classification. It

would not suit Mill very well, I think, because he would want to list also ethics, logic, and psychology. Comte would probably regard ethics as a part of sociology, and would regard that part of psychology which is scientific as coming partly under biology and partly under sociology. That part of psychology which is not scientific--i.e., that part which employs introspection--would be of no interest to Comte since he would regard it as metaphysical and so would regard it as having no place within positive knowledge.[183] Comte seems to have left logic out of his list on the ground that logic is the methodology of science and does not constitute a separate science. If we mean by logic the operations of the human mind this can be investigated by biology and sociology. Mill, too, undoubtedly sees logic as the methodology of science. Yet I think he would list it as a separate science.

Another consideration which Mill might have taken into account if he had attempted a classification of the sciences would be the nature of their subject matter. This would also yield a serial arrangement very similar to Comte's. It would probably begin with algebra as having the least empirical "content" and end with the sciences of human and social behaviour as having the most.

I find Mill's view on the possibility of sciences of human and social behaviour interesting, and I should like to point out briefly what he says. Mill refers to these sciences by the generic term of moral sciences, and lists under that heading psychology, ethology, sociology, history, politics (which in-

cludes what is today called economics), and morality. In the Logic it is the possibility of such sciences which concerns him. The possibility of such sciences depends in part upon developing methods suitable to them, and in part upon the constancy of human behaviour. Is human behaviour constant in the sense of being subject to invariable causal laws? In a chapter of the Logic called, "Of Liberty and Necessity," Mill attemps to establish two points which have to be taken into consideration whenever we attempt to decide the relation of human behaviour to its causes. The first is that human character is undoubtedly a result of causes. Mill makes the point that among these causes may be our wish to change our character, and holds that even though this wish may be the result of environmental circumstances, the fact remains that we possess such a wish. Mill accepts a doctrine of circumstantial conditioning but rejects fatalism, or a rigid determinism:

> I saw that though our character is formed by circumstances, our own desires can do much to shape those circumstances . . . ; that our will, by influencing some of our circumstances, can modify our future habits or capabilities of willing . . . . From that time I drew in my own mind, a clear distinction between the doctrine of circumstances and Fatalism; discarding altogether the misleading word Necessity.[184]

A second point which Mill seeks to establish is that the causal series which lie at the basis of human personality and behaviour are not all resolvable into series which have either pleasure or pain, or the anticipation of these, as the causal elements in the series. If we call the causal factors "motives," then it is necessary to acknowledge other motives than

the desire of pleasure and the desire to avoid pain. Although behaviour may have a hedonistic basis in the beginning, eventually we come to will certain acts without any reference to their being pleasurable. We have, in addition to likings and aversions, purposes in life. And these purposes are, in many cases, the determining factor in volition.

The science of human nature seems to Mill to be analogous to the science of the tides, rather than to a science such as astronomy. For the science of human nature is still in its early stages, where specific laws have yet to be made out. We are far from being close to its general laws. Moreover, the science of human nature is difficult to formulate for the reason that the data are very difficult to determine. They are both extensive and complex. And they are frequently "hidden," so that while we may be convinced that given the data we could formulate their laws, to get at the data is sometimes impossible. Furthermore, given certain laws of behaviour, it is difficult to apply them because we are not always certain that the case before us completely resembles the cases from which the laws have been arrived at inductively. This is not to say that there are not absolute laws relative to human nature. But they are so only in the sense that we can say that such and such will be the effect of a given cause when it is acting freely, or so far as it operates uncounteracted. Human behaviour, however, results from the conflict and interaction of numerous laws. Even so, there is no reason why we should not have laws which would enable us to predict,

with a high degree of probability, how humans will behave. These would be statistical laws. We are further hindered in formulating a science of human behaviour by the difficulty, and in most cases the moral impossibility, of performing experiments. The most we can hope for, then, are general laws based upon observation--in other words, empirical laws--which we test by means of the deductive method, that is to say by determining the consequences of such general laws and finding out the truth of these deductions by returning to concrete experiences.

Mill sees psychology as forming the basis of ethology--the science of the individual man, or the science of character--with ethology being a deductive science based upon psychology. Mill seems to mean by ethology--a science which he tells us in his day did not exist--what is meant today by applied psychology.

The science which deals with man in socially organized groups Mill calls political or social science. It differs from psychology and ethology primarily in the matter of complexity. The laws operative here are so complex, according to Mill, that to grasp their interaction completely is beyond our powers. Not only are the factors which determine the group behaviour of a society numerous, but they are also constantly changing. Mankind cannot hope to master the interrelations of these factors to the extent that we can use our knowledge to control the behaviour of societies. But we can at least use our knowledge of political and social science

for the guidance of society, and for the determination of tendencies. It is particularly in a complicated subject such as social science that Mill advocates the concrete deductive method. The "chemical" method he rejects because it presupposes that the entities involved take on new characteristics when they enter into combination. The laws of water are different from those of its component elements. But, says Mill, in the case of men, men are not converted into another kind of substance when they unite for social living. The laws of the behaviour of individual human beings are, ultimately, the basis of the laws of society. Where social science is concerned our problem is not so much to discover new laws as it is to find means to evaluate the results of the composition of causes. Many causes are at work in society and the problem is not so much to determine the causes as it is to determine what their effects will be. This problem tends to make the "geometrical" method of no use to the social scientist. In geometry we are never faced with the problem of conflicting causes, of causes which counteract or modify one another.

For this reason Mill criticizes those writers who assume that all social phenomena flow from a single force, or from a single property of human nature. Social science, as Mill sees it, must be a deductive science, not after the model of geometry, but after the model of the more complex physical sciences, as, for example, astronomy. Each effect, both in astronomy and in Mill's proposed social science, is the result of numerous causes. The behaviour of human beings in society

is held to be governed by the laws of psychology and ethology. It is not too difficult to determine the effect upon society which the operation of any one of these laws, acting alone, might have. But to attempt to infer, from the compounding of all these laws acting simultaneously, what the effect will be in any given case, is a problem surpassing human ingenuity. Nevertheless, something, at least, can be done, Mill feels. By continually verifying in concrete experience, the results of proposed general laws, certain general laws can be established with some degree of probability.

Mill suggests, borrowing the idea from Comte, that occasionally an "inverse deductive method" be employed. Here, instead of deducing our conclusions by reasoning and verifying them by observation, we begin with conclusions based on specific experience and attempt to link these conclusions with the laws of psychology and ethology. The laws of psychology and ethology in this case constitute the empirical verifications. The verification consists in deducing from the laws of human nature the sociological law inferred from historical data. Comte, for example, regards social science as consisting of generalizations based upon history, and verified, not originally suggested, by deductions from the laws of human nature. Mill thinks there is considerable room for the employment of the direct deductive method in sociology, but agrees with Comte that nothing of a really scientific nature can be achieved in the area of social science except by the employment of the inverse deductive method.[185]

Mill had some insight into the role that statistical techniques were to play in determining the laws of social behaviour. But for the most part the significance of social statistics for him is that they provide evidence in support of the doctrine that human behaviour comes under the law of invariable causation. If human behaviour does not come under this law then, of course, sciences of human behaviour become impossible.

9. Philosophy

To attempt to discover the manner in which a particular philosopher views philosophy as a kind of knowledge is, perhaps, a peculiar interest of my own and not entirely germane to the present thesis. However, I favour the idea that a properly constructed classification of the sciences must contain, as one of the disciplines classified, that discipline which classifies the sciences. I feel that any discussion, or analysis, of knowledge must include within the kinds of knowledge that discussion or analysis. What, then, is Mill's attitude towards philosophy? What is philosophy for him, and what is its status among the kinds of knowledge?

Although I have not discovered any passage in which Mill refers to himself specifically as a philosopher, it seems certain that he regarded himself as dealing with knowledge, or science, in a rather special way. He is said to have maintained, in a letter written in 1831: "If there is any science I am capable of promoting, I think it is the science of sci-

ence, itself, the science of investigation of method."[186] And, while he may have refrained from calling himself a philosopher, I should think he would agree that he engaged in philosophizing. In the preface to the first edition of the Logic we find a characteristic use of the term "philosophize":

> To cement together the detached fragments of a subject, never yet treated as a whole; to harmonize the true portions of discordant theories, by supplying the links of thought necessary to connect them, and by disentangling them from the errors with which they are always more or less interwoven; must necessarily require a considerable amount of original speculation. To other originality than this, the present work lays no claim. In the existing state of the cultivation of the sciences, there would be a very strong presumption against any one who should imagine that he had effected a revolution in the theory of the investigation of truth, or added any fundamentally new process to the practise of it. The improvement which remains to be effected in the methods of philosophizing . . . can only consist in performing, more systematically and accurately, operations with which, at least in their elementary forms, the human intellect in some or other of its employments is already familiar.[187]

"Philosophizing," in the above passage, seems to refer simply to the systematic pursuit of knowledge, the pursuit of knowledge requiring the systematic and accurate performance of certain intellectual operations. And Mill seems to regard his function to be to give clarity and precision to these operations. In this sense, then, we may regard the Logic as an essay in philosophizing, and as dealing with science on its theoretical, or methodological, side. And we can, perhaps, regard a work such as Utilitarianism as an attempt on Mill's part to be a practising philosopher, an attempt to employ the method he recommends.

Mill has a tendency to use the terms "science" and "philo-

sophy" interchangeably. Philosophy, or philosophizing, appears, in part, to be for Mill a search for principles and laws. And he uses the term "philosopher" to denote almost any one whose aim is to discover general laws. The following passage indicates this attitude:

> The doctrine . . . that the collective series of social phenomena, in other words the course of history, is subject to general laws, which philosophy may possibly detect--has been familiar for generations to the scientific thinkers of the Continent.[188]

But apparently Mill is loath to use the term to denote those whose aim is solely to apply general rules. Like Whewell he is inclined to feel that for philosophy the goal is knowledge for its own sake and that this distinguishes philosophy from art. Although as noted above, Mill fails to make any very clear distinction between science and philosophy, one cannot help but feel when reading him--especially when reading the Logic--that there is an implicit recognition of a distinction between a writer such as Kepler and a writer such as Mill himself, between the practice of science and talk about science. But Mill does not make it explicit, and he undoubtedly regarded himself as contributing to science, whatever may have been his feelings about himself as a scientist.

Furthermore, Mill recognizes metaphysics as a unique inquiry of some kind. The metaphysician, in Mill's view, elaborates those causes which the logician merely posits as primitive. There are numerous instances in the Logic where Mill asserts that he need not investigate such and such for the reason that "this is a problem for the metaphysician." But

it is my opinion that Mill's <u>Logic</u> involves, and, in fact, includes a metaphysics, even though he claims that it does not.[189] Mill's categories seem to me to denote ontological kinds of things--substances, that is to say, bodies and minds, being the most obvious. If he had meant by "body" and "mind" simply certain arrangements of sensations or feelings, he might, perhaps, have avoided metaphysics. But he clearly means by "body" and "mind" something not given within consciousness:

> Body having now been defined the external cause, and (according to the more reasonable opinion) the <u>hidden</u> external cause, to which we refer our sensations; it remains to frame a definition of Mind. Nor, after the preceding observations, will this be difficult. For, as our conception of a body is that of an unknown exciting cause of sensations, so our conception of a mind is that of an unknown recipient, or percipient, of them; and not of them alone, but of all our other feelings. As body is the mysterious something which excites the mind to feel, so mind is the mysterious something which feels and thinks . . . . On the inmost nature of the thinking principle, as well as on the inmost nature of matter, we are, and with our faculties must always remain, entirely in the dark.[190]

Now, if we are to employ the terms "body" and "mind" meaningfully, and Mill clearly thinks that we can do so, it will be necessary to say that the referents of these terms are known but not perceived. It further seems to be the case that Mill does not recognize, or does not accept, the possibility of defining matter and mind operationally. Instead, he refers to each as a "mysterious something."[191] And Mill's other categories--feelings and attributes (quantities, qualities, and relations)--might certainly be called metaphysical, if not precisely ontological, provided we define "metaphysical" to mean irreducible realities, or the most general classes

of things which can be named. The elements so known are metaphysical in the sense of being first principles. As such they are preinferential. There is no logic, no methodology which is applicable to them. For example, speaking of resemblance, which is a relation, Mill says: "Resemblance between two phenomena is more intelligible in itself than any explanation could make it."192

I will now leave the problem of metaphysics, and return to the question, Would Mill consider his own writings philosophy? I think he holds that there is a certain body of knowledge which is common to all knowledge, but which is itself not precisely physics or mathematics or astronomy or psychology or what have you. Mill calls it logic, or the science of science. To call this body of knowledge philosophy is awkward for the reason that Mill sometimes means by philosophy the natural sciences. And he sometimes means by philosophy the whole of knowledge. Whether or not what Mill calls logic be called philosophy is of no great moment. But I do think he regards it as a separate discipline and that he would think any proper classification of the sciences ought to include it.

10. Certainty and the "Whole" of Knowledge

The matter of certainty has traditionally been the mark of knowledge. Whewell begins his inquiry with kinds of knowledge which are certain, their certainty being their guarantee that they qualify as knowledge. Mill does not proceed in this way. Secondly, Whewell sees something peculiar about the cer-

tainty of axioms, a peculiarity which he traces to their subjective origin. He then goes on to assert that a branch of knowledge can become a science when it can be set up in such a way that it can be shown to follow from its axiomatic basis. But this is not Mill's way. The certainty of axioms is not a peculiar certainty. And geometry, in its method, is not necessarily a model for any other science. Whewell and Mill would both agree that whatever we can claim to know, we know with certainty. For Whewell certainty is associated with the totality of knowledge. For him we cannot be said really to possess knowledge so long as there is some truth of which we are ignorant. This seems to me an orientation derived from Hegel. To know is to know all. And anything short of this cannot be said really to constitute knowledge. Apart from a complete whole, or system, of knowledge, into which each piece fits, there is no true knowledge of the pieces. But Mill seems little impressed by such a view:

> Any facts are fitted, in themselves, to be a subject of science, which follow one another according to constant laws.[193]

CHAPTER VI

THE DATA OF KNOWLEDGE ACCORDING TO MILL

I wish to discuss the data of knowledge under three headings: the data of intuition, the data of induction, and the data of deduction.

A. The Data of Intuition

The data of intuition, for Mill, are called by the generic term, "feelings." Feelings include emotion, volition, and sensation. The first two are not of much interest as far as the present thesis is concerned. But the third--sensation--is of great interest.

It is easy enough to restate what Mill says about sensations. But it is very difficult to be certain what he meant. My view is that the obtuseness of Mill's account of sensations springs in large measure from his failure to indicate, unambiguously, the spatial locus of sensations. If we use "here" to denote the perceiver, and "there" to denote the objective world, then in Mill's case, are sensations "here," or "there"? I find that Mill has four different answers to this question. And I have to admit that I do not see how these four "answers" can be resolved into a single theory of sensations. These answers, as I understand them, are stated in physiological terms, psychological terms, epistemological terms, and logical terms.

I shall deal with these in order.

Sensations are "here" in that they are physiological processes taking place within the epidermis of the perceiver. In this sense sensations are known only to the physiologist, and are of no interest to the logician. I cannot see what Mill stands to gain by introducing this approach to sensations. For (1) sensations as so defined are not perceived. They cannot, therefore, be said to constitute that prelogical truth which Mill is seeking. And (2) as so defined sensations lose that qualitative aspect which is one of the major bulwarks of Mill's whole approach to knowledge. Mill is constantly reiterating the necessity of recognizing qualitative distinctions. For example, the laws of nature, he says "can never be resolved into one ultimate law," because of the fact that our sensations, which are the basis of the laws, fall into groups which are qualitatively distinct. States of consciousness which are qualitatively distinct are, to that extent, <u>sui generis</u>, and cannot be explained by an appeal to the laws of other qualitatively distinct experiences:

> For example; since there is a phenomenon <u>sui generis</u>, called colour . . . it follows that there are ultimate laws of colour . . . . I do not mean that it might not possibly be shown that some other phenomenon, some chemical or mechanical action for example, invariably precedes, and is the cause of, every phenomenon of colour. But though this, if proved, would be an important extension of our knowledge of nature, it would not explain how or why a motion, or a chemical action, can produce a sensation of colour and however diligent might be our scrutiny of the phenomena . . . the last link would still be a law of colour, not a law of motion, nor of any other phenomenon whatever.[194]

In view of the opinion expressed in the above quotation,

I cannot see why Mill sometimes extends the term "sensation" to include "a motion," or "a chemical action." At most it merely enables him to point out that sensations _in this sense_ are of no interest to the logician.

Sensations are "here" in the second sense for Mill in that they are constitutive of consciousness. This may be said to be a psychological approach to sensations. Psychology studies consciousness, and sensations make up part of consciousness, and, therefore, part of the subject matter of psychology. And again Mill declares that as logician he has no interest in sensations in this sense either. His subject matter is not the content of consciousness. It is, instead, valid thinking. Furthermore, the grounds of valid thinking are not our conscious states _per se_. Instead, the grounds of valid thinking are such rules as _nota notae est nota_.

To make this second distinction, the proverbial hair has to be split pretty finely. But I think the distinction is defensible. All that can be said of a state of consciousness is that it is here-now. And my reading of Mill leads me to the conclusion that, for him, the evidence for the _truth_ of inductive statements is not, _e.g._, "green patches here-now." Green patches here-now, at most serve two functions. (1) Such particulars may be wholly constitutive of objects. (Later I shall give some reasons as to why I think they may _not_ be wholly constitutive of objects.) And (2) particulars serve as the referents of those statements whose truth we grasp intuitively. Mill, of course, does not use such an expression as "green

patches here-now." But he does use statements which refer to the content of consciousness. And these provide a basis for the whole of knowledge. They are basic because they are known, intuitively, to be true. Moreover, since they are intuitively known to be true, their truth "carries over" into the realm of inference. We could have no confidence in our inferences unless we were certain that our preinferential statements were true. And the only way of proving that preinferential statements are true is to "refer" them to the content of consciousness.

On what ground, then, can it be said that the logician has no interest in the conscious states studied by the psychologist? One approach is to say that there is no ground for such a statement, since it is obviously untrue. Granted, Mill asserts the statement. But then he immediately appears to contradict himself. However, I think two considerations can be pointed out which indicate that Mill does not really contradict himself on this issue.

One consideration I have already suggested: the immediate--i.e., preinferential--content of consciousness is the ground of inferential statements. But taken purely in and of itself the preinferential content of consciousness could only be the ground of such statements as "Green is green" or "Green is here-now." It could not be the ground of a statement such as, "I see a green patch." Because, for Mill, the "I" denotes an inferred subject (provided "I see a green patch" be taken as an item of first order knowledge only.) Nor could

the content of consciousness alone give rise to an induction. Because an incuction proceeds from the known to the unknown, and the content of consciousness is always merely immediate. In knowing it, we know it truly. We know that it is. And we know it as it is. But that is all we know about it. And that is all we <u>can</u> know about it without appealing to some other ground than the immediate content of consciousness itself.

A second consideration is this. When it comes to the matter of determining the principles of evidence, the logician legislates to the psychologist, and not <u>vice versa</u>. The psychologist, it is true, is the one who knows the laws of human behaviour. And thinking is one kind of human behaviour. From the standpoint of psychology, one of the fundamental sets of laws relating to thinking are the laws of association. Those laws are capable of telling us what we think and what the temporal origins of our thoughts are. But they are not capable of telling us what we ought to think. Therefore, even if it be the case--and I must say that the commentators whom I have read universally hold that it is the case--that my thoughts can be "reduced"--<u>i.e.</u>, traced back to-- the primitive elements of consciousness--nevertheless I hold Mill's view to be that this reduction cannot tell me which of my thoughts are "true." The reduction may tell me, for example, <u>why</u> I think the earth goes round the sun or why I think the sun goes round the earth. But it does not tell me whether the earth really does, or does not, go round the sun. It does not, in other words, tell me whether "the earth goes round the sun" is a true

statement. Granted, the evidence for the Copernican theory is sensory experience. But the evidence for the Ptolemaic theory is also sensory experience. And qua sensory experience, all such evidence is equally coercive.

Sensations are "there" as distinct from "here" for Mill in that they are present to, and, therefore, are objective to, something which knows that they are present and that they are sensations. This is an epistemological approach to sensations. Sensations are elements within a theory and a metatheory of knowledge.

The theory of knowledge in which they appear holds that knowledge consists in relating particulars, and that some of the particulars so related are sensations.

The metatheory of knowledge in which they appear justifies the above theory. It attempts to prove that there "really are" sensations. And it attempts to prove that to relate sensations is to produce knowledge. It shows--or attempts to show--that statements in which names of sensations appear are "real" statements and it shows that such real statements are "important." A statement is "real," for Mill, if it is synthetic and if it is capable of empirical verification. A statement is "important" if it is a real statement and if it answers our questions about experience and nature, thereby furthering our desire to know. These are the usual meanings of "important" for Mill. But a third meaning occurs from time to time: A statement is important if it has pragmatic value.

Sensations are "there" in another sense for Mill in that

(1) they are perceived as present in objects which, in turn, are perceived as being other than the perceiver. And (2) they can be predicated of objects. This view of sensations, although it is in part ontological, is primarily logical. It is primarily logical because the logical problems of predication are discussed in terms of sensations.

To the extent that this view has any support, the support has to be derived primarily from the _Examination of Hamilton_. And it is a view which I find very much at odds with the view set forth in the _Logic_. In the _Logic_ Mill does not say that the attributes of objects are, _qua_ attributes, sensations. Instead, he explicitly denies that this is the case. White, for example, is an attribute of snow. And whiteness is a quality which snow shares with milk. But neither white, nor whiteness, are sensations. In order to refer to the relevant sensation here, we have to employ a circumlocution and speak of the sensation _of_ white, or the sensation _of_ whiteness. The statement, "Snow is white," is, therefore, not about a sensation. Our sensation, in this case, Mill tells us, does not have a name. For this reason, the view, held by most commentators, that, for Mill, objects are just bundles of sensations, and can be reduced to sensations, I find highly suspect, the book on Hamilton notwithstanding.[195]

I am, however, willing to allow that the commentators have some ground for what they say. But there is one crucial item missing from their case. In order to establish such a case it is first necessary to establish that Mill held that

sensations are known as being "out there" and that they are where they appear to be. But I have not read a single commentator who either establishes, or attempts to establish, this crucial item. For example, in anything of Mill's that I have read, he held the <u>sensation</u> of whiteness to be "in here." But the snow, and the whiteness of the snow, he held to be "out there." Objects and their qualities, therefore, in Mill's case, cannot be reduced to sensations until someone first shows that Mill held sensations to be "out there." And I do not think it can be done. Such a view of sensations, namely, that sensations are "out there," might be a very reasonable view of sensations. But I do not think it is Mill's view. What are "out there," for him, are objects, attributes, and relations. And I do not think these can be "reduced to" something "in here." If one wants to know what is "out there" one asks a physicist, or an astronomer, or a geologist. If one wants to know what is "in here" one asks a psychologist. All a psychologist can tell us about what is "out there" is that we believe there is something out there, and why we believe that there is. The justification for this belief has to be provided by the philosopher. The philosopher tells us not why we <u>believe</u> there is something out there, but why we <u>think</u> there is something out there. We think there is something out there because (1) we have sensations. (2) As a result of experience, which involves memory as well as sensation, we conclude that everything must have a cause. Therefore, sensations must have causes. (3) If we define "cause" tempor-

ally, then one sensation can be the cause of another. But such a theory produces paradoxes like night being the cause of day. The only way to avoid such a paradox is to postulate permanent causes. (4) Experience and nature--but not immediate sensation--have the quality of perdurability. Perdurability can only be understood in terms of enduring substances. Therefore, there are enduring substances. Such, as I see it, is the structure of Mill's argument here. (I underline "argument" to distinguish it from his appeal to immediate perception. Immediate perception also assures us of the presence of objects. But it is the above argument in which I am interested at the moment.) And how the above argument can be reversed so that we end up with nothing but sensations escapes me.

The epistemological account of sensations in Mill is the one which most interests me. Feelings are immediately present to consciousness. Whatever is known immediately by consciousness is known beyond possibility of question. Feelings, then, are the data of intuition. And propositions which are about feelings are known to be true on the evidence of intuition. No science is required for the purpose of establishing such truths. No rules of art can render our knowledge of them more certain than they are in themselves. There is no logic for this portion of our knowledge.[196] For this portion of our knowledge we have the direct evidence of consciousness. And, "according to all philosophers, the evidence of Consciousness, if only we can obtain it pure, is conclusive."[197]

B. The Data of Induction

1. The Kinds of Inductive Data

There are three kinds of data upon which the method of induction may be employed. These are feelings, objects, and what I shall call abstractions.

I have already discussed feelings as the data of intuition. And they do not change their character when they are taken as data of induction. I see no need, therefore, to discuss them further in the present context.

I have also said about all I want to say about objects. Objects are not among the data of intuition. Instead objects are the result of an activity called inference. The act itself is not within our present consciousness or memory. Nor does Mill give any clues as to what characterizes the act. Perhaps we should have to go to the psychologists to find this out. I do not know. (In fact, I think Mill's position would be stronger if he had reversed his argument: objects are given, sensations are inferred.) At any rate, inductive knowledge of objects, and of the properties of objects, makes up the major portion of knowledge. Objects, which are among the data of induction, are the result of inference. And I am reasonably certain, as I intimated above, that objects are not reducible to sensations, and that the act of inference, which yields objects, is something other than the operation of laws of association combining sensations.

But while I fail to see how all objects can be reduced to

sensations, Mill does attempt to show, especially in the Examination of Hamilton, that statements about objects can be translated into statements about sensations. In such a context an object becomes a permanent possibility of sensations and perhaps this is the equivalent of "reducing" objects to sensations. Even so, I find it difficult to conceptualize a "permanent possibility" in sensationalistic terms. Mill claims to mean by it only that if I were to place myself in such and such a position, I should have such and such sensations. But even if it is Mill's final view that knowledge of objects is not irreducibly distinguishable from knowledge of actual and possible sensations--and I am willing to grant that this might be his view, since it may be I who am out of step here, and not the rest of the regiment--there are two kinds of objection to this view which can be raised. (1) In the first place one might bring to bear certain external criticisms. One might argue, for example, that the distinction between object and sensations is a necessary distinction. And if Mill does not observe it so much the worse for Mill. (2) One might state a number of internal criticisms. For example, (a) one might point out that, if, in the end, Mill abandons the distinction between sensations and objects, then many of the questions raised in the Logic will have to go unanswered. (b) One might point out that logic, on such a view, becomes merely a part of psychology. This would give rise to a great number of contradictory statements within the Logic, to the effect that logic is, and is not, only psychology. It would

also make the Logic, qua logic, an object lesson in bad logic. (3) One might point out that there is a large class of cases in which one would not know how to go about reducing objects to sensations. Since Mill fails to provide a translation demonstrably applicable to all cases one could maintain that, for Mill, there remains a knowledge of objects which is irreducibly distinct from knowledge of actual and possible sensations.

A third kind of data for induction is that kind arrived at by abstraction. The mind, Mill tells us, is able to attend to part of the data before it. This is the process which yields the data of the "abstract" sciences. I have already said a good deal about data of this kind. And I shall say more about them when I discuss the method of deduction. Therefore, all I shall do in the present paragraph is point out that there are such data.

2. The Data of Induction and What Is

A difficult problem in Mill, as I have indicated above, is whether or not the data of induction are exhaustive of what is. As nearly as I can make out, the answer is "No." At this point Mill, qua logician, is prone to hand over the problem to the metaphysician. As a logician this is probably his privilege. But Mill's logic is so solidly set within the broader framework of epistemology that he really cannot avoid these issues, and usually ends by discussing them. Furthermore, his opinion that in writing the Logic he was contributing to the science of science frequently impresses him

with the necessity to make his doctrines square with scientific ones. It seems to have been his view that a knowledge of temporal sequences is all we <u>need</u> to know of causes. But on the question as to whether or not temporal sequence is all we <u>can</u> know of causes, he wavers. It was clear to him that all we can establish and prove is temporal sequence. Yet scientists all around him were using "cause" in the dynamic sense of force, and were claiming to have a knowledge of this force. Furthermore, he himself saw the need for something permanent, something possessing the quality of "perdurability". And so he attempts to decide what sort of meaning can be given to a word like "matter," which seemed to him in physics to account for force and perdurability. His problem here essentially is to define "matter" denotatively. And this is a great problem because experience is solely of phenomena. And while as a logician he is able to say that the logician need not concern himself with forces which are not phenomena, it is clear that he feels the epistemologist must give some account of them. As an epistemologist he has to be careful that he does not assert propositions which are incapable of proof, since he does not cease to be a logician by becoming an epistemologist. Nevertheless, whereas at the beginning of the <u>Logic</u> he is able to be disdainful of the supersensible world, by the end of it he has shifted his ground somewhat. By the time that he reaches the end of the <u>Logic</u> the possibility of knowledge seems to require the assertion of supersensible existence. But all we can do is assert it. The true, because

the only really available, subject matter are phenomena. Yet, in addition to discovering and proving the laws of phenomena, it is necessary to account for them also in terms of their existence, "existence itself . . . being . . . the power of producing phenomena."[198] Now, since matter may be similarly defined as the power of producing phenomena, I presume we can consider matter to be one kind of existence.

In one sense matter is unknowable, in that it is not a phenomenon, nor a group of phenomena. But in the eighth edition of the Logic we read:

> The indestructibility of Force no more interferes with the theory of Causation than the indestructibility of Matter, meaning by matter the element of resistance in the sensible world.[199]

Now this is quite a different approach to matter. We are not now asking what matter may be in the supersensible world but, rather, What is matter for us? Even so, surely Mill speaks carelessly in the above passage? He can hardly mean that matter *is* the sensation of resistance, but, must instead mean that the sensation of resistance is a sign from which we infer the cause of the sensation. At any rate, we see here one of the many problems which it is one of the intentions of the book on Hamilton to resolve. Resistance can be a datum of induction. Existence *per se* cannot.

C. The Data of Deduction

Mill employs the term "deduction" in two distinguishable ways. And in order to indicate the nature of the data employed by deduction it is necessary to point out the two mean-

ings which deduction may have.

1. <u>Deduction Exemplified by the Syllogism</u>

One type of deduction is exemplified by the syllogism. This type of deduction does not particularly interest Mill, since it is neither a method of discovery nor of proof. It is merely a method for setting down in a clear, formal way what we already know. This is not to say that the syllogism is useless. Its employment guides our interpretation, and application, of general rules found out by induction. The syllogism does not analyze the process by which the general rules are arrived at. But it does facilitate our applying the law to a specific instance in a manner consistent with the conclusions which form the basis of the generalization. The syllogism does not prove, for example, that since all men are mortal the Duke of Wellington is mortal. The proof that the Duke of Wellington will die does not originate in any syllogism having "All men are mortal" as its major premise, but rather in those individual experiences which lead us to conclude that the attributes connoted by the term "man" are marks of mortality. But the syllogism does prevent our arriving at conclusions about individual cases which would be inconsistent with the evidence which gives rise to the general rule. It performs a further valuable function in that it sometimes brings us face to face with the fact that either we have made some erroneous judgment about a particular case, or else that our general rule, or law, is faulty. If, then,

we mean by deduction, the employment of the syllogism, the data of deduction are inductive generalizations such as, "All men are mortal" and descriptions such as, "The Duke of Wellington is a man." Mill does not require both premises of a syllogism to be general.

2. The Concrete Deductive Method

The second type of deduction Mill calls the concrete deductive method. It is the method to be preferred when we are attempting to extend our knowledge of nature. This method has two major uses: to prove the laws of phenomena and to explain them. The method consists of three operations: induction, ratiocination, and verification. The problem with which this type of deduction attempts to deal is the determination of the effects which will follow from certain causes. The method itself will be discussed briefly later. At the moment it is the data which the method employs which I wish to indicate.

The inductive part of the method employs "the data of induction," discussed above. This part of the operation attempts to discover causes, the laws of those causes, and their tendencies. The second part of the method takes as its data, the laws provided by the inductive operation, and, by means of thought estimates the combined tendencies of the various laws. The problem here is, Given a certain combination of causes, what effects will follow? It is of great advantage to the inquirer if, at this stage of the inquiry he can bring mathematics to bear in his calculations. The third step in the

method, verification, employs as data direct observation, wherever direct observation is available. Where direct observation is not available, we may employ various kinds of indirect verification. For example, a future event which comes to pass in accordance with a given prediction would constitute a direct verification. But often we should have to wait too long for such verification. In such a case we may resort to indirect verification. For example, we may find that if we had employed the conclusions we have now arrived at, we _could have_ predicted past events. A third type of verification occurs when from truly causal laws we can deduce what had previously been held as merely empirical laws.[200]

CHAPTER VII

THE METHODS OF KNOWING ACCORDING TO MILL

For Mill there are three basic methods of knowing: induction, deduction, and intuition.

A. The Method of Induction

In order to expound Mill's account of induction it is necessary to examine many complexities associated with induction. One could begin with these complexities, or end with them. I plan to end with them. But the result of leaving them until the end is that any preliminary statements about induction will subsequently have to be modified.

Induction is inferring something unobserved from something observed.

> Induction . . . is that operation of the mind, by which we infer that what we know to be true in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects . . . . Induction is the process by which we conclude that what is true of certain individuals of a class is true of the whole class, or that what is true at certain times will be true in similar circumstances at all times.[201]

In very general terms, there are two methods which can be called inductive. One is induction by simple enumeration. The other is "scientific" induction.

1. Induction By Simple Enumeration

Induction by simple enumeration <u>rests on</u> positive instances. It <u>consists in</u> ascribing the character of general truths to all propositions which are true in every instance that we happen to know of. But Mill is extremely vague when it comes to describing whatever it is that accomplishes the transition from the noting of positive instances to asserting a generalized statement. This kind of induction, he says, is "natural." "The unprompted tendency of the mind is to generalize its experience, provided this points all in one direction."[202]

Mill's account of induction by simple enumeration is, then, little more than an historical account. First, we observe positive instances, and then we assert a generalization. We note that all the swans which we have seen have been white, and then we assert that all swans are white. And the only attempt Mill makes over and above this historical account, to describe the method of induction by simple enumeration, is to assert a "natural propensity to generalize." The problem of how we "go from" statements about ~~statements~~ particular observations to generalized statements seems to have interested Mill very little.[203] His interest lies with the problem of establishing the truth or falsity of the generalizations. "The business of inductive logic is to provide rules and models . . . to which if inductive arguments conform those arguments are conclusive and not otherwise."[204]

2. <u>"Scientific" Induction</u>

The task of the logician is to state the conditions under

which the evidence which supports an induction is conclusive. With "scientific" inductions, as with induction by simple enumeration, Mill has relatively little interest in the method which actually yields the generalization. This is not to say that he has no interest in this problem. But he is primarily interested in the matter of the evaluation of evidence. He is not so much interested in where such statements as "All swans are white," and "All men are mortal" come from. He is, instead, interested in techniques for establishing, on the basis of a limited enumeration, that the latter is, and the former is not, universally and necessarily true.

In working out the techniques of proof in this area, Mill, implicitly changes his orientation towards induction.[205] As long as we regard induction as a generalization from experience, we can never be certain that some exception to the generalization will not turn up. We can never be certain that the very next instance which we examine will not go contrary to our past experience. Mill's solution to this difficulty--a solution which is not as carefully worked out as it should be--is, implicitly, to redefine induction. Induction is not a generalization from experience. It is, instead, an analysis of experience. The scientific endeavour now becomes to discover marks of marks, to discover marks which invariably go together. Induction now rests not on invariable connection, but upon necessary--or, as Mill calls it, unconditional--connection. If we can discover that the marks "man" and "mortality" necessarily go together, and that the marks "swan" and

"white" do not, then we have solved the problem of induction.

When redefined in this way, induction still remains an appeal to experience. But on the basis of the definition we can establish that there are more and less effective ways in which to appeal to experience. The principle nota notae est nota, guides us in formulating a method which will make best use of the principle. This method Mill calls the concrete deductive method. It is discussed briefly in the material below.

3. Empirical Laws Versus Laws of Nature

Induction is the method whereby we derive laws from our experience of nature. The laws are of two kinds. One kind Mill calls "merely empirical." The other kind he calls laws of nature, or "truly causal" laws. Merely empirical laws are the result of simple enumeration. And their proof can be established by the positive instances from which they have been derived. (Or their proof can be said to be established by the Method of Agreement.) Laws of nature are generally the result of what was called, above, scientific induction. Such laws are generally proven by the employment of the Four Methods. But they are not always the result of scientific induction. Instead, they are occasionally the result of simple enumeration. When they are the result of simple enumeration they are not proven by the Four Methods, but by the positive instances from which they have been derived. An example of such a law is the law that every event has a cause.

If we were certain that all empirical laws were derivable from laws of nature, then there is no reason why empirical laws should not have the same necessity as laws of nature. But we very easily see that many empirical laws may not be derivative from laws of nature, but from chance occurrences, *i.e.*, from what Mill calls "collocations." For example, we discover that on the earth coal was formed during a certain geological epoch. But, if there is coal on other planets, we cannot know, without going there, whether or not it was formed during the same epoch as that in which coal was formed on earth. So while, in this case, the empirical law depends in part on laws of nature, it also depends in part on certain collocations which may have been peculiar to the earth. Here, then, is a law which we cannot extend beyond the limits of time and place and circumstance in which we actually know it, by direct inspection, to be true. In general, the criterion of an empirical law is that the only evidence on which it rests is that obtained by the method of agreement. Therefore, almost all truths arrived at by simple observation must be regarded as empirical laws until either confirmed by the method of difference or explained deductively. (This does not always hold in the case of truths arrived at by experiment, as opposed to truths arrived at by observation). It might also be noted that empirical laws differ among themselves with regard to their degree of certainty. It is highly likely, for example, that the history of development of each frog is the same as the history of any other. It is highly unlikely that

the beds of stratified rock on Mars--if there are stratified rocks on Mars--were laid down in the same order as those on the earth.

In the end, empirical laws play a very important and unique role. Although themselves very much in need of "explanation" by means of deduction, they become, in the final analysis, the most impressive means of verifying such deductions.[206]

Mention must also be made of those cases in which induction by simple enumeration amounts to proof. Let us take for example, the law of causation. How is the universality of this law established? That every phenomenon must have some cause, some antecedent upon the existence of which it invariably and unconditionally follows, is basic to all that Mill has to say about induction. What proof is there of this law? That is to say, on what evidence does it rest? The law, itself an induction, undoubtedly results, Mill tells us, from a generalization based on many laws of a lesser degree of generality. However, since all induction in the strict, or scientific, sense of the term presupposes the general law, our knowledge of the particular inductions which form the basis of the law must have some other basis than the general law itself. If we make the specific laws the basis of the most general one, we cannot properly make the general one the basis of the specific ones. We are, therefore, forced to admit that prior to the time that the most general law was formulated, the specific laws which subsequently formed its basis could only

have rested on induction by simple enumeration. And if this is the basis of the specific laws it is equally, therefore, the basis of the most general law.

However, just because the law of universal causation rests ultimately on simple enumeration, it does not follow that it is insecurely based. For, according to Mill,

> the precariousness of the method of simple enumeration is in an inverse ratio to the largeness of the generalization. The process is delusive and insufficient, exactly in proportion as the subject matter of the observation is special and limited in extent. As the sphere widens, this unscientific method becomes less and less liable to mislead; and the most universal class of truths, the law of causation for instance, and the principles of number and geometry, are duly and satisfactorily proved by that method alone, nor are they susceptible of any other proof.[207]

The largeness of the generalization minimizes the deficiencies of the method of simple enumeration in the following manner. The larger the generalization the greater likelihood there is of its being shown to be false, if it is false, and the greater the number of instances that will appear in support of it if it is true. In the case of the law that every phenomenon is preceded by some characteristic event which is its cause, we have innumerable instances to prove that it is true, and none to prove that it is false. We are, therefore, entitled to assume that what has held in such a great number of cases, without exception, will hold in all possible cases. The major ground for the induction is, undoubtedly, in Mill's view, the great number of positive instances. But our confidence in the induction springs from the belief that since we have extended the generalization to include the whole of na-

ture, if there were exceptions we should have come upon them long ago.

I think that one weakness in Mill's argument is a failure to attempt to prove that all phenomena are necessarily alike in kind. Unless he does this his claim that the universality of causation is empirically grounded is not very well established. He admits that we have no way of knowing that the law holds in distant parts of the universe, because there may be phenomena there unlike those with which we are familiar. But in his claim that all phenomena with which we are familiar are alike, it seems to me he begs the question. He also occasionally gives us some cause to wonder whether or not he actually believed all phenomena are alike. But with the exception of this particular problem I have little internal criticism to offer of Mill's account of the empirical ground of the law of universal causation. I do not claim that Mill has established his case. But I concede the skill of his argument.[208]

4. Induction and the Concrete Deductive Method

I now wish to discuss induction as the basis of the concrete deductive method. This is a very complicated matter and requires not only an examination of the inductive method but also an examination of a number of "operations subsidiary to induction," as Mill calls them. These subsidiary operations, especially the operations of observation, description, abstraction, the formation of conceptions, naming, and classi-

fication, are of greater interest to me than are, for example, what are generally called Mill's Methods. But before I begin to set forth these subsidiary operations, I should like to include a brief account of induction _per se_. There is a most important issue here, namely Mill's claim that all knowledge is basically inductive.

Since this is an important issue I wish to elaborate some of the things which I have already said about induction. The first point is that Mill's _Logic_ treats logic as simply the science of reasoning, meaning by reasoning both the inference of any assertion from assertions already admitted and the testing of such inferences. As part of the science of reasoning in the present sense the canons of induction are probably intended to be both rules of discovery and methods of proof:

> Logic . . . is the science of the operations of the understanding which are subservient to the estimation of evidence: both the process itself of advancing from known truths to unknown, and all other intellectual operations in so far auxiliary to this.[209]

But the major task for logic is that of proof. Logic does not provide proofs, but methods of proof. It teaches the conditions to which facts must conform if they are to prove other facts, but it does not itself teach which facts prove which others. To do so would be to usurp the roles of the particular sciences. In this sense, then, we may affirm that logic is, as Bacon says, the science of science itself. And since science and, indeed, since all knowledge which is not intuitive, is a product of inference, logic, for Mill, becomes both an analysis of the process of inference and of the

rules, or canons, for testing the validity of inferences. Mill holds that the process of passing from particular observations to particular conclusions without an intermediate generalization, is just as valid an instance of induction as is the more usual process of passing from particular propositions to more general propositions.

It is necessary to distinguish induction from operations which are sometimes confused with it. The operation most frequently confused with induction is description. On this ground Mill rejects as examples of induction such assertions as "All the apostles were Jews," and Kepler's conclusion that the orbit of Mars is an ellipse. Such propositions are mere descriptions and do not satisfy the definition of induction as an inference from facts known to facts unknown. True inductions do not describe facts, they arrange facts, explain facts, and predict facts, i.e., determine the circumstances under which similar facts will recur. In Mill's view the goal of induction is to discover laws of nature, and, having discovered laws, to base predictions on them, or, as he phrases it, to follow them into their results. He tries, without too great success it seems to me, to distinguish certain uniformities in nature which are truly laws of nature from others which are not truly laws of nature. The criterion of the distinction seems to be that a uniformity is not a law of nature if it can be deduced or inferred from other uniformities. It is not a law of nature that mercury will rise in the tube of a barometer, but it is a law of nature that air has weight. Given

the law, and certain other laws which deal with the behaviour of fluids under pressure, the rise of mercury in the tube of a barometer could be deduced. Strictly speaking then, the laws of nature are the fewest, simplest, most general laws from which the whole existing order of nature could be seen to result. Or they might be said to be the fewest general propositions from which all the other uniformities in nature could be deduced. However, for all practical purposes Mill seems to make little use of the above distinction, and the goal of induction may be said to be to discover any uniform sequence which nature exhibits.

Induction begins either with observation or experiment, the latter being much to be preferred whenever it can be employed. The first major problem which we encounter when we attempt to employ induction is that the universe, at first glance, presents the appearance of a multiplicity of phenomena Yet we believe that there is order here. We believe that every event is necessarily linked to at least one which precedes it. But which ones are linked to which? The easiest way to get an answer to this question is to vary the circumstances. Sometimes nature does this for us. In such a case we may employ observation. In others we can vary the circumstances ourselves, that is to say, conduct an experiment. The major advantage of the experiment is that by means of it we can manipulate causes, whereas in observation we are limited to noting effects. Observation, then, can yield a knowledge of invariable sequence, but it cannot yield a knowledge of

unconditional sequence.[210]

Personally, I think Mill is floundering a bit when he attempts to make this distinction. What he is trying to distinguish here are two kinds of series, a causal series, as for example, that curare will cause death, and a purely temporal, but not necessary causal, series, as for example, that night follows day. I agree that the distinction is valid. But it is a difficult distinction to make in the light of Mill's definition of cause. This is not to say that he fails to make the distinction clear. One can show that the taking of curare is the condition on which death is, in certain cases, dependent, so that we have here a true example of causation. In the case of night and day, once we know of the rotation of the earth we can explain night and day as being each the effect of the earth's rotation as cause. But in the absence of a knowledge of the earth's rotation, and since the nature of the case is such that we cannot perform an experiment but have to rely solely on observation, the most we could say would be that day is the invariable antecedent of night. We could not say whether or not it is the cause of night, that is to say, that it is the <u>unconditional</u> antecedent of night. Perhaps Mill's terms "invariable" and "unconditional" do solve his problem satisfactorily. But I cannot help feeling that the force of his argument rests to some extent on the reader's willingness to make, unconsciously, a distinction which Mill does not explicitly ask him to make--the distinction between a truly causal and a merely temporal series. This seems to me

letting in at the back door the concept of cause, as force, which was thrown out at the front. If it were not the case that everybody knows there is a distinction between a truly causal and a merely temporal series, I doubt that Mill could explain, without modifying his definition of cause, the manner in which we can distinguish an invariable antecedent from an unconditional one.

The problem, then, for induction, comes to this. How can we single out from the many circumstances which precede or follow any given phenomenon, those with which it is invariably or unconditionally related? Mill's answer here is twofold. In the case of some phenomena as, for example, those of astronomy, or of human and social behaviour, we are forced to rely merely on observation, and here the concrete deductive method is to be employed. In the case of other phenomena we can employ experimentation, and here Mill offers his Four Methods of experimental inquiry, methods which he cannot be said to have originated, but which he sets forth with great clarity.

The Four Methods themselves are of no particular interest to the present thesis. They are, as I see them, essentially ways of extending and of guiding observation, since what is known by means of them is ultimately known directly. They do not, at least they do not claim to, proceed by means of what Whewell calls hypotheses. Mill restricts the use of hypothesis to the "hypothetical" method, which is a variation upon the concrete deductive method. But they may be employed to test hypotheses. Mill has little to say about the origin of hypo-

theses and seems to regard the trial and error method as some sort of natural, almost instinctive, way of proceeding. Finding a right hypothesis is, for him, like our behaviour when we attempt to find an article which we have misplaced. He defines an hypothesis as any supposition which we make, either without actual evidence, or on evidence admittedly insufficient, in order to try to deduce from it conclusions in accordance with known truths. The underlying supposition is that if the conclusions to which the hypothesis leads are true, the hypothesis itself either is true also, or is highly likely to be so. And, an hypothesis being a mere supposition, the only limits to hypotheses are those of human ingenuity. But, in any case, it is verification which constitutes the proof of the hypothesis, not the ability to base deductions on it. And by verification is meant not only the ability to deduce certain laws of natural phenomena, but evidence which establishes the reality of the cause asserted by the hypothesis. Otherwise we cannot be said to establish the cause of the phenomena.

Hypotheses are invented in order to enable us to apply the deductive method earlier than would otherwise be the case. In other words, the hypothetical method dispenses, temporarily, with the first step of the concrete deductive method--i.e., the hypothetical method dispenses with the induction which determines the laws of the causes. Therefore, the hypothetical method is acceptable only when the verification amounts to, and fulfils the conditions of, a complete induction. Newton,

for example, in proving that the force which at each instant deflects a planet from its course, and makes it describe a curve around the sun, is a force tending directly towards the sun, began with this proposition as an hypothesis. And this hypothesis he subsequently established as a law of nature by employing the method of difference.[211]

Such, in very brief fashion, is Mill's concept of the method of induction. It can be pictured as an observer looking on at a constantly changing panorama, and recording such regularities or uniformities as he is able to observe or produce. The end products of induction are laws, either of the merely empirical or of the scientific variety. The former exhibit varying degrees of probability, for the most part. But occasionally we are able to regard them as certain. Scientific laws are true. Our knowledge that they are true is partly derivative from the methods employed in attaining them, partly derivative from the fact that they are capable of empirical verification. But probably no law is held with quite the degree of assurance which characterizes our knowledge of our subjective states. And, for this reason, these remain the final court of appeal and the certainty which characterizes them the ultimate criterion of truth.

5. The Operations Subsidiary to Induction

It now remains to give some account of the operations subsidiary to induction, for these serve to make clearer the nature of that knowledge which induction provides.

(a) Observation

One operation, observation, has already been discussed to some extent. To modern writers, a major problem concerned with observation is to avoid what Bacon calls "idols of the cave" or, in modern terms, to produce, or establish, "normal operators." In other words, the problem is to see what is really there and to eliminate the idiosyncrasies and errors which may be introduced by the individual observer. This is also of concern to Mill. But he approaches the problem in a fashion peculiar to himself. The major problem, he asserts, is not with the actual sensory material, but with the inferences based upon it. Most of what we claim to observe actually consists of inferences, and in the matter of inferring we may easily make mistakes. He does not suggest, however, as some of the British Empiricists seem to do, that we dispense with the inference and accept as observable data only the undeniably certain material of consciousness. Instead, he advocates that we keep clearly in mind whether or not we are talking about our conscious states or about our inferences. When disagreement arises it is usually on the level of inference, not on the level of sensation. And the validity of inferences can be checked by the employment of the methods of experimental inquiry.

I must say, however, that there seem to me many problems here which cannot be solved by an appeal to the Four Methods. To take a simple example, there are many cases in which the sensations of any two observers may differ. In such cases, I

cannot accept Mill's view that the solution of the differences is to be found at the level of inference. Thus, of all the sensations different persons may have of a penny, which is the "right" one? The methods of experimental inquiry may resolve disagreement as to whether or not curare is poisonous. But I cannot see that they could resolve disagreements such as whether or not a penny is really round, a wall is really green, and so forth. The problem of establishing public, observable, objects seems to me a most difficult one. But it does not seem to have appeared so to Mill. He does have a brief discussion of the problem in his book on Hamilton. However, for the most part he asks, What is the ground of my belief in an external world? rather than, What reason is there to suppose that there is a public world? The ground of belief in an external world is, in the first place, an awareness that even when particular groups of sensations cease, the possibility of their recurrence remains. This possibility is independent of our will and of our presence. Secondly, there is the observation that other human beings also have sensations and these sensations are external to me. But Mill's whole argument merely assumes, as far as I can determine, that other people have the "same" sensations that I have. At this point Mill comes close to a form of naive realism.

(b) <u>Description</u>

A second process subsidiary to induction is that of description. And just as observation always goes beyond that which is strictly observation, our observations generally being

in terms of objects rather than in terms of our sensations, so description contains something which is not, strictly speaking, description. What we attempt to describe is something discrete. But in describing it we employ terms which denote or connote many other phenomena than the one we are attempting to describe. We cannot, therefore, describe without classifying, that is to say without asserting a resemblance between the phenomenon before us and something else.

(c) Abstraction

A third process subsidiary to induction is abstraction. This process concerns itself with the realm of ideas, particularly abstract ideas--that is to say, the kind of idea called up by a general name. Mill does not attempt to expound the nature of such ideas, but merely asserts that we have them, and that induction would be impossible if we did not. A general idea represents in the mind the class of things to which a name is applied. And whenever we think about the class, or reason about it, we do so in terms of the general idea. General ideas arise in the mind as a result of comparing phenomena which are, in fact, alike, by means of what Mill calls the power of abstraction. This is the power which the mind is said to have, of attending to part of the data before it. By its means the common element shared by the data can be selected and named. Although the name could include the idea among its referents, Mill usually accepts the common sense approach, and applies the name to the object. Thus, for example, we

apply the word "white" to white objects, not to our ideas. The whole process of selecting the common property and naming it, is to some extent arbitrary. We might, for example, classify animals on the basis of colour into black animals, white animals, and so forth. But from the standpoint of scientific induction it is necessary that we group things into classes which are really classes, that is to say, into classes grounded on distinctive, truly significant, points of agreement shared by the members. What will constitute truly significant points of agreement will depend in part on the characteristics of the phenomena, but also in part on the purpose which the classification is intended to serve. Moreover, it is necessary that things be grouped into classes of which something "important" can be asserted.

Mill's subscription to natural classes is clear enough, but how such classes are to be identified is not at all clear. It is my opinion that he relies on an intuition of the essence of the things classified. He tells us, for example, that some of the distinguishing marks of man are the possession of life, reason, two legs, and a certain physical shape. But apart from the suggestions offered above, that these are "truly significant" and that of them "something important" can be predicated, he offers us little in the way of argument in defence of the "truly significant" features which he selects as definitive of man. The grounds on which he rejects other attributes as definitive is equally vague. There seems to me no obvious reason why mortality should not be as much a

part of the essence of man as is the possession of life, and no obvious reason why the possession of thirty-two teeth should not be as important as the possession of two legs:

> The concept Man is not the sum of all the attributes of a man, but only of the essential attributes--of those which constitute him a man; in other words, those on which the class Man is grounded, and which are connoted by the name--what used to be called the essence of Man, that without which Man cannot be, or, in other words, would not be what he is called. Without mortality, or without thirty-two teeth, he would still be called a man: we should not say, This is not a man; we should say, This man is not mortal, or has fewer than thirty-two teeth.[212]

Mill's doctrine of Abstraction came under considerable fire from his contemporaries, and by the time we reach the eighth edition of the Logic, some modification of the doctrine can be discerned. I doubt whether his original position underwent any great change here. But the expression of it is more careful. For example, he makes an attempt to refrain from using "concept" and "idea" as technical terms, although he still retains them as literary ones. It is clear also in the book on Hamilton that Mill makes a greater attempt to set forth in terms of sensations and names--or to show how they could be so set forth--propositions earlier stated in terms of ideas and concepts. Thus, where we find him saying, in the fifth edition of the <u>Logic</u> that reasoning proceeds by means of general ideas, in the book on Hamilton we read:

> To say . . . that we think by means of concepts, is only a circuitous and obscure way of saying that we think by means of general or class names . . . . I consider it nothing less than a misfortune, that the words Concept, General Notion, or any other phrase to express the supposed mental modification corresponding to a class name, should ever have been in-

vented. Above all, I hold that nothing but confusion ever results from introducing the term Concept into Logic, and that instead of the Concept of a class, we should always speak of the signification of a class name.[213]

(d) Naming

Another process upon which induction depends, and one already touched upon to some extent, is that of naming. Mill asserts that induction is not absolutely dependent upon the operation of naming. It can proceed in the absence of names. But it would be induction of a low order, an inference of one particular fact from another. Mill claims that animals are capable of performing this type of induction. However, without language induction would be limited to very simple cases, because it would have to be that sort of induction which can take place without the employment of general propositions. Language also functions as an artificial memory enabling us to work out trains of thought which the memory, unaided by language, could not hold before itself constantly and accurately. In order to perform an induction of any significance it is necessary that the induction apply to a whole class of cases. And, in order to perform the induction it is necessary to hold all the cases before the mind at once. This we could not do if we were unable to record, in the form of signs, those similarities and uniformities which we observe between similar cases. We take cases A and B, discover the ways in which they are alike and employ signs to signify these similarities. We then examine A and C, and repeat the process.

Eventually our signs become general signs signifying what have been called universals. They become general names which may be used in general propositions. And it is upon general names that we are dependent for every meaningful proposition. It must, however, be recognized that Mill does not fully succeed in working out a satisfactory solution to the logical problems which plague him at this point. At the end he is still trying to bring together two kinds of denotata: concepts and phenomena.

Part of the difficulty here, he tells us, results from a deficiency of language. We do not, strictly speaking, have names for our sensations. Instead we have names for things, and names for the qualities, or attributes, of things.

Where Mill is really in difficulty, I feel, is in the matter of the relation of language to thought. I think that he fails to make, or at any rate, to maintain, a sharp enough distinction between the process of thinking and the results of thinking, between science as an intellectual activity and science as a body of organized knowledge, between the results of science viewed as beliefs, thoughts, and the like, and the results of science as something which can be written down and placed between the covers of a book. And, although it may be Mill's wish to leave thinking, <u>per se</u>, to the psychologists, he finds himself obliged to give some account of the process of thinking. Hence no matter how strongly he may assert that the term "concept" should not appear within logic, the fact of the matter is that he himself cannot get along without it.

For example, having asserted, in a passage quoted above, that we think by means of general or class names, we find Mill saying twenty pages later, "A Concept . . . is the mental representation formed within us of a phaenomenon; or rather, it is a part of that mental representation, marked off by a sign, for a particular purpose."214

Mill also fails to observe, apparently, that the meaning of the word "concept" may shift when it is transferred from one context to another, and that there may be good reason to allow it to do so. For a logician a concept is simply that which can be subject or predicate of a proposition, whereas for a psychologist a concept is a mental event. And much of Mill's difficulty can be traced to his refusal to permit the term "concept" to have this ambiguity. Instead he tries to use it univocally within a logical context, a psychological context, and a metaphysical context. Hence the concept which is the subject of a proposition must be the same as that concept which is referred to in the quotation immediately above as a mental representation. And for this reason the name of the object, or the name of a class of objects, can denote both the object, or class of objects, and the mental representation. If, instead of the above attempt at a solution Mill had pointed out an inadequacy in language here similar to the one referred to above in relation to sensations, and had insisted that our ideas lack names just as much as do our sensations, he might have found a solution to some of his more vexing problems.

In order to fulfil the requirements of induction there

are, says Mill, two major conditions which the names we employ must satisfy. The first is that every general name which we employ should have a specific meaning. The second is that we should have a name available whenever there is something important to express. Where the matter of precise definition is concerned Mill points out the necessity of putting into the connotative definition the essential features of the thing named. But he contents himself for the most part, with quoting large sections of Whewell's discussion of the same problem. Where the matter of securing all the terms of which we have need is concerned, Mill claims that a satisfactory philosophical language consists of three essential parts: names for describing the individual facts observed, a name for every common property of any significance, and a name for every kind of thing. By a kind is meant a class of things which is distinguished from other classes not by a few properties but by an unknown number of properties. In the case of a kind no definition could exhaust the properties common to its members. Thus "horse" signifies a kind, but "white horse" does not.

Mill discusses briefly, and rejects, the possibility of employing language in such a way that we might reason by means of it mechanically, as is done in mathematics, especially in algebra. The symbolic language of mathematics can never, Mill says, become the ideal type of philosophical, and scientific, language. Mathematical terms denote the barest minimum of empirical data, and when we use the terms we have no specific object in mind. Arithmetic deals chiefly with number, and

is true of all phenomena to the extent that they are numerical, but is able to tell us nothing about the things themselves. In algebra the most we have to hold before our minds is that things which are equal to the same things are equal to one another and that the sums or differences of equal things are equal. Sciences like arithmetic and algebra can proceed by rule, because they do not need to concern themselves about things. But induction cannot proceed by rule, because in induction it is necessary constantly to refer to actual phenomena in their concrete wholeness. While the meaning of "a" in an algebraic equation is of little significance, and could mean anything without affecting the validity of the reasoning, in other sciences the situation is quite the reverse:

> It is, in short, as necessary, on all subjects not mathematical, that the things on which we reason should be conceived by us in the concrete, and 'clothed in circumstances,' as it is in algebra that we should keep all individualizing peculiarities sedulously out of view.[215]

I should like to say something in Mill's defence at this point. Those critics of Mill who regard themselves as spokesmen for the scientific point of view, have had some very nearsighted things to say, it seems to me, about Mill's attitude towards the desirability of quantitative data. And the point I should like to make is that Mill is fully aware of the importance of such data. Mill points out that mathematics is the greatest of all agents for transforming an inductive, experimental, science into a deductive one. It is not the desirability of quantitative data that he questions, but the

feasibility of reducing all propositions, without loss of meaning, to mathematical ones. To say that "Poetry is better than pushpin," or that "The sky is blue" can be stated mathematically is simply nonsense. Mathematics may say something meaningful about poetry, pushpin, the sky, and the colour blue, but it cannot state all that is to be said about them. Any complete knowledge, or satisfactory analysis, of experience will have to include terms capable of handling qualitative distinctions as long as experience itself exhibits such differences.

(e) Classification

The final operation, subsidiary to induction, of which I wish to speak is classification. There is, as has been pointed out above, a type of classification inseparable from the process of naming. Here the classification follows unintentionally. Our purpose is to name not to classify. But in science the major desire frequently is to classify, and in such a situation naming becomes a secondary consideration. Our purpose in establishing a scientific classification is to bring order into our view of objects, to divide up the whole of nature in such a way that the interrelations of the objects which constitute it may be made more apparent and, where they are not apparent, may be more easily discovered. With these goals in mind we try to classify objects in terms of those properties which are causally significant. Ideally we should establish each class in terms of the cause of the peculiarities of that class. But since such causes are frequently not apparent, we attempt to classify in terms of the more prominent effects of

such causes, which serve as marks of the other effects and of the cause. Such a classification may be termed a natural, as opposed to an artificial, classification. This is not to say that, to obtain certain practical ends, objects may not be classified in a variety of ways. But where our purpose is purely to extend our knowledge of nature the most natural classification is that which rests on those properties of objects which contribute most to render the objects within any group like each other and unlike the members of other groups. A further consideration is that all natural kinds must have a place within the classification. But it is not necessary that all the classes be kinds.

Another form of classification which Mill stresses is one discussed by Comte, viz., classification by series. Induction being inquiry into the laws of some phenomenon, it is desirable to bring into one class all the kinds which exhibit that phenomenon and to arrange the kinds according to the degree to which they exhibit the phenomenon. Such a classification is very useful in the study of comparative anatomy, for example. This type of classification is closely allied to the employment of the method of concomitant variations which presupposes, as has been stated earlier, that phenomena which vary together, and disappear together, are either cause and effect or effects of a common cause. Therefore, the establishment of a classification according to series often greatly facilitates the subsequent employment of the method of concomitant variations in determining the laws of the phenomena.

b. The Method of Deduction, and the Concrete Deductive Method

1. The Method of Deduction

Mill, like Whewell, insists that deduction cannot properly exist apart from induction. But he goes beyond Whewell in his claim that all knowledge is, in the final analysis, inductive. Deduction, for Mill, is a device which makes induction a more powerful instrument and which, when allied with induction, becomes what he calls the concrete deductive method--the method which, in his view, will become <u>the</u> scientific method.

Deduction rests on induction in a variety of ways. In the first place induction supplies what are usually referred to as the axioms which deduction presupposes. It determines the meaning of the concepts and terminology which deduction employs. It supplies the broad generalizations from which deductive argument begins, and the lesser generalizations which constitute a major proof of the end products of deduction.

The syllogism symbolizes the form of all deductive argument, since any deductive argument can be cast into the form of the syllogism. It is not the case, however, that any reasoning actually goes on within the syllogism. The conclusion of an argument set up in syllogistic form is not drawn from the major premise, but in accordance with the major premise. The syllogism is not the form in which we must reason, but it is instead a form in which we may set forth the reasoning the better to examine its validity. Syllogistic reasoning, then, is a form of deductive reasoning, but it is not a neces-

sary form, since deduction occurs whenever we interpret a general psoposition. Mill, therefore, regards every process by which anything is inferred respecting an unobserved case as consisting of an induction followed by a deduction. And the value of the syllogism, like the value of the methods of experimental inquiry, lies not in the fact that we must employ such methods in order to reason at all, but rather that we must employ them in order to determine whether or not our reasoning is correct.

As to how deduction actually proceeds, this may, I think, be stated fairly briefly. It has usually been said, Mill asserts, that the foundation of the syllogism--that is to say, the justification of the claim that conclusions reached _via_ the syllogism are valid--is the _dictum de omni et nullo_, the dictum that whatever can be affirmed, or denied, of a class, may be affirmed, or denied, of every member of the class. Mill, however, rejects this idea, primarily on the ground that universals are not, as was generally supposed when the dictum was formulated, some kind of substance having an objective existence independent of the existence of the individual objects classed under them. Such a doctrine served a useful purpose in that it implicitly asserted the intercommunity of natural objects, and was of real significance, Mill tells us, at a time when, for example, "man" meant something other than all men. But, he goes on, now that it is known that a class, universal, species, or genus is not an entity _per se_, that a class is nothing but the objects contained within it, the _dictum de_

omni et nullo is seen to be nothing other than a circuitous definition of the word "class," and amounts to no more than the tautological statement that what is true of certain objects is true of each of them. Mill proposes as the foundation of the syllogism that we substitute for the dictum de omni et nullo the two principles that things which coexist with the same thing coexist with one another, and that a thing which coexists with another thing, with which other a third thing does not coexist, does not coexist with that third thing. Deduction, therefore, proceeds by determining the presence or absence of such coexistences. However, the above two principles can be more satisfactorily stated as one principle:

> Whatever possesses any mark, possesses that which it is a mark of. Or, when the minor premise as well as the major is universal, we may state it thus: Whatever is a mark of any mark, is a mark of that which this last is a mark of.[216]

In the light of the above principle the syllogistic form may be modified as follows:

> "Attribute A is a mark of attribute B,
> The given object has the mark A
> therefore
> The given object has the attribute B."[217]

There are, Mill holds, good reasons for attempting to give to every science as much of the character of deduction as possible. Our ability to deduce the truths of a science from a relatively small number of inductions gives to the science a simplicity and a closeness of structure which Mill finds highly desirable. Such a science does not become any

the less inductive, but it does become less experimental. And the major distinction between the sciences, Mill states at one point, is not between deductive and inductive sciences, but between deductive and experimental sciences. A science is experimental to the extent that the laws of each new fact have to be established on the basis of a new induction. A science is deductive to the extent that it can draw conclusions concerning new phenomena by processes which bring these new cases under old inductions. Thus mechanics has been rendered deductive by bringing it under the general laws of mathematics. And astronomy has been rendered deductive by bringing it under, or within, the laws of general mechanics.

There are two major ways in which an experimental science may become deductive. One is by continued experimentation, where repeated inductions link together previously isolated phenomena. For example, we may begin by knowing that "a" is a mark of "b," and "c" a mark of "d." A subsequent induction may establish that "b" is a mark of "c" which enables us to demonstrate deductively that "a" is a mark of "d." The major problem here then is to discover marks of marks, for this is what permits the employment of deduction.

But the most useful agent for transforming experimental into deductive sciences, is the science of number. Of all the properties of things, number alone is a property of all things. Mathematical truths, of course, apply to all things only to the extent that they are numerable. But, even so,

> if it comes to be discovered that variations of quality in any class of phenomena, correspond regularly

> to variations of quantity either in those same or in some other phenomena; every formula of mathematics applicable to quantities which vary in that particular manner, becomes a mark of a corresponding general truth respecting the variations in quality which accompany them: and the science of quantity being (as far as any science can be) altogether deductive, the theory of that particular kind of qualities becomes, to this extent, deductive likewise.[218]

2. The Concrete Deductive Method

I have very little to add to the brief account of the concrete deductive method given above under the heading "The Data of Deduction." As stated there the concrete deductive method consists of induction, ratiocination--generally speaking, deduction--and verification.

Astronomy provides a paradigm of a science which employs this method. The individual facts on which astronomy grounds its most important deductions--such facts as the size of the bodies of the solar systems, their distances from one another, the shape of the earth, and its rotation--are not accessible to direct observation. Instead they are established indirectly, by the employment of inductions based on facts which are accessible to observation.

An example of the employment of the method is the proof that the central force of the solar system is identical with the force of gravity. First the law is established that the earth attracts the moon with a force varying as the inverse square of the distance. (Although this law is partly dependent on prior deductions, it is essentially inductive in that it establishes the law of the cause.) Secondly, from this

law, and from other data--*e.g.*, the mean distance of the earth from the moon, and a variety of other items--it is determined how rapidly the moon would fall to the earth if there were no extraneous forces acting other than those which act upon terrestrial bodies. This is the second step, the ratiocination. Finally, the calculated velocity is compared with the observed velocity--16 feet per second per second--with which terrestrial bodies fall, by mere gravity, towards the surface of the earth. And the two velocities are found to agree.

The hypothetical method is a variant of the concrete deductive method. It substitutes an hypothesis for the first of the above three operations. This method is entirely legitimate, Mill says, provided the final step, the verification, amounts to, and fulfils the conditions of, a scientific induction. We want to be assured that the law we have assumed, as an hypothesis, is a true one. This assurance is secured if we can deduce true results from it, provided we can also establish that no law, except the one we have assumed, can lead to the same conclusion. This *proviso* can often be met. For example, in establishing that the central force of the solar system is identical with the force of gravity, Newton began with an hypothesis, namely, that the force which deflects a planet from its rectilinear course, and makes it describe a curve around the sun, is a force tending directly towards the sun. He then proved that if this is the case the planet will describe equal areas in equal times. And, finally, he proved that if the force acted in any other direction whatsoever,

the planet could not describe equal areas in equal times. Having thus established that no other hypothesis would be consonant with the facts, the hypothesis was proven. "The hypothesis became an inductive truth."[219]

It is worth noting, in relation to the concrete deductive method, the way in which Mill finally "returns to" experience:

> The ground of confidence in any concrete deductive science is not the _a priori_ reasoning itself but the accordance between its results and those of observation _a posteriori_.[220]

As noted earlier, much of Mill's interest in the concrete deductive method arises in his conviction that by its employment truly scientific social sciences would be possible.[221]

C. The Method of Intuition

The philosophers whom we today might call rationalists, Mill generally calls intuitionists. And, in a number of areas, he is violently opposed to the appeal to intuition on which such philosophers ground their more sweeping generalizations. However, if we mean by "intuition" an immediate awareness, then belief in intuition is basic to _everything_ Mill says. In the present chapter I wish to discuss Mill's attack on the intuitionists, and his own employment of intuition. Mill's attack on the intuitionists I shall discuss under the heading, "Knowledge of Axioms." Mill's employment of intuition I shall discuss under the headings, "Psychology and Intuition," "Theory of Knowledge," and "Metatheory of Knowledge."

1. Knowledge of Axioms

Mill states as forcefully as it can be stated that all

those propositions which some writers call axioms, and which they claim to be intuitively valid per se, can be shown to be inductions.[222] As far as any proof of such axioms is concerned, Mill is possibly right. But there remains a possibility which Mill does not examine, namely that the axioms may be incapable of empirical proof. Perhaps they can be seen to be necessary. But it may reasonably be doubted whether, on the basis of the empirical evidence which supports them, we can prove them to be true.

(a) The Axiom of Equality

From the standpoint of modern writers, Mill's concept of the axiomatic basis of mathematics is, no doubt, primitive, and probably not of much interest. But primitive or not, it is an integral part of his philosophical system. And I think it can be shown that his view is unsatisfactory without having to bring in twentieth-century writers to show what the axiomatic basis of mathematics really is. My concern is not to establish the real basis of mathematics, but to examine what Mill says about axioms.[223]

The basic axioms of mathematics, according to Mill, are the axioms of equality. Mill describes these as the most simple and obvious inductions. He seems to me to have two major arguments in support of this claim. One is that regardless of the source of our knowledge of axioms, our grounds for believing them, that is to say the nature of their verification, is inductive. We accept them as true because there is

evidence in support of them. The second argument is that there is some sort of insight, or perhaps experience--in the cumulative sense of experience--that any given belief and its contradictory are mutually destructive if we attempt to combine them, and that, therefore, the laws, or axioms, of thought in general are true:

> That the same thing is and is not--that it did and did not rain at the same time and place, that a man is both alive and not alive--are forms of words which carry no signification to my mind . . . . One half of the statement simply . . . takes away the meaning which the other half has laid down. The unmeaningness here resides in the copula. The word is, has no meaning except as exclusive of is not.[224]

In his discussion of arithmetic Mill states that '1 = 1' is the fundamental proposition on which the truth of all arithmetical propositions and procedures depends.[225] It seems to me, then, that Mill is asserting that '1 = 1' is the axiom on which arithmetic rests. If so, I think this is a specious argument. It would have made a better argument if Mill had held that the basic axiom of arithmetic is simply that of equality of which '1 = 1,' '2 = 2,' and so forth are subsidiary inductions forming the basis of the fundamental generalization. At any rate, by means of inductions we discover the basic relationships which hold between numbers, numbers being properties of real things. Arithmetic, then, is an inductive science in origin, its basic axioms being elaborated by means of deduction.

But since, as Mill acknowledges, '1 = 1,' '2 = 2,' and so forth, hold true of phenomena only to the extent that phenomena are numbers, this does not give to the axiom that if

equals are added to equals the results are equal, the validity Mill claims for it apart from mathematics. He admits that one actual pound weight is not exactly equal to another, one actual mile is not exactly equal to another. It still remains to be shown what is the induction, and what are its data, from which axioms of equality are derived outside arithmetic. In geometry, for example, Mill holds that we always deal with actual lines, either physical lines or lines presented to the imagination. And how we are to determine that such actual things are equal, I am sure I do not know. Mill suggests that we determine such physical equalities by means of measures, i.e., by means of footrules, balances, and the like. But I think the difficulty cannot be removed in this way. For, by the employment of a measure, we arrive at a number, and thereby we return to arithmetic. In so doing we still leave the substantial quality of the phenomena unmeasured. For example, if I measure two boards with a footrule and discover that each board is 6 feet long, all I have measured is the length of the boards in terms of mathematical units. As a result of certain operations I have arrived at a number. But this in itself will not even prove that '6 = 6,' let alone provide a datum for some non-mathematical induction concerning the equality of physical things as, for example, that the two boards, above, are the same length. As nearly as I can make out, then, Mill fails to account for the universality of the axiom of equality.

(b) The Axiom that All Things Are Numerable

Another instance of a proposition which I think we may say Mill regards as an axiom is the proposition, "All things possess quantity; consist of parts which can be numbered; and in that character possess all the properties which are called properties of numbers."[228] This proposition is, presumably, an induction from sensory experience. It is not a description because it includes an inference from things known to things unknown. It is my opinion that Mill cannot establish such a principle inductively. The principle is, I think, a proper epistemological or methodological axiom. But its necessity may lie in its being prescriptive rather than descriptive. In order for it to be established inductively it first has to be shown that the external world is atomic in its makeup. And it seems to me that the atomicity of the external world is an inference, not a datum in Mill's case.

Mill's views on mathematics have come in for a great deal of unfavourable criticism, and I dare say that most of it is justifiable. I do not know that he regarded himself as a mathematician, and perhaps he was unwise to have quite so much to say about the subject. Yet, at the same time, his philosophical position would be incomplete without some account of the nature and origin of mathematical truths. His motive for entering the field of mathematics seems to have been not so much a feeling of competence as a mathematician but, rather a desire to show "the intuitionists," as he calls them, a thing or two. Since mathematics was their ultimate

stronghold, Mill could not claim a victory for himself as long as the bastion of mathematics remained unassailed. But Mill is very cautious about the success of his venture.[227]

I do not consider it necessary to enter further into the controversy that I have done in the above paragraphs. But I should like to point out something which the critics I have read have overlooked. Mill states, quite unequivocably, that geometry is not literally true, meaning thereby that it does not accurately describe the physical world. (As far as I am concerned, if this be conceded there is no sense whatever in attempting to establish its truth inductively.) The only reason we can suppose it to be absolutely true, Mill goes on to say, is if we suppose that it does accurately describe the physical world. If we allow that its axioms are true of the objective world, then the rest of geometry follows. If Mill had only altered his position slightly he might have said something profound here, because he was close to an important contemporary orientation which Einstein states as follows:

> In my opinion . . . as far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.[228]

Mill seems to have satisfied himself of the first of Einstein's propositions--that in so far as the laws of geometry apply to reality, they are not certain. But in his account of the fact that they are nonetheless certain he demonstrates that, where mathematics is concerned, he is no Einstein. For he tries to make them certain in relation to geometrical

figures held before the imagination from which the real geometrical objects are abstracted and in relation to which geometrical propositions are true. What he might have done, given his predilection for induction, was to have argued for a truly inductive geometry as the temporal antecedent, rather than as the evidential ground, of deductive geometry. This would yield a theoretical geometry which is absolutely true _qua_ geometry, just as theoretical physics is absolutely true _qua_ theoretical physics, but which does not hold absolutely when we come to apply it to the empirical world. In its application to the empirical world such a geometry would remain empirical in the sense that one could not determine beforehand what results one would get if one were to measure a triangle drawn on a piece of paper. But it would be no more empirical than physics in this regard.

(c) The Axiom that Nature is uniform

I should like to discuss only one more of Mill's axioms, the axiom of the uniformity of nature. It is asserted by the rationalists, for example, by Whewell, that the axioms of knowledge cannot be inductively arrived at because induction cannot establish their universality and necessity. I have discussed above what Mill has to say about the inductive basis of the universality of the axioms, and I should like now to discuss his views concerning their necessity. Can we show inductively that the axioms are necessary, that is to say that they not only are true, but that they must be true? Mill proceeds cau-

tiously here, asserting that the burden of proof lies with his adversaries. And for the most part he contents himself with showing that they have not succeeded in demonstrating that the necessity of the axioms can not be accounted for on the basis of induction from experience. However, Mill's own position can be pieced together from his criticisms, especially from his criticisms of Whewell.

The principle of the uniformity of nature can be stated in a variety of ways, but for my present purpose the best way to state it seems to be to say that there are no uncaused events. I have already stated the manner in which the law of the uniformity of nature is established, and I shall not repeat these arguments. But there are at least two points which Mill has not discussed fully enough, and to which I have not previously referred. Mill takes as definitive of what Whewell means by the necessity of a law of nature, and especially by the necessity of an axiom as sweeping as that the course of nature is uniform, the impossibility of conceiving its contradictory to be true. And in attempting to refute Whewell Mill comes close to undermining his own position by dangerously mixing the grounds of our belief in a law with the evidence for the law.

2. Psychology and Intuition

Mill translates the impossibility of which Whewell speaks into psychological terms.[229] And at the end of this discussion it begins to look very much as though the induction which Mill holds to be the basis of the law of the uniformity of nature

is none other, in fact, than an act of psychological conditioning. We have seen so many instances of causal series that we finally arrive at a state where we cannot believe that an event may have no cause. If this is Mill's final position, two important consequences follow. The first is that the law of the uniformity of nature is not an induction concerning external nature at all, but a description of our psychological make-up. The second consequence is that on such a basis the reason we experience no events as uncaused is because we are so thoroughly conditioned in the causal view of things that we would not recognize an uncaused event if we did come upon one. The absence of negative evidence to the law, therefore, may not rest in the nature of the data, but upon the psychological make-up of the observer.

Now the above two consequences would be disastrous to Mill's system because it depends for its validity upon the observer's seeing truly what is there to be seen. And even if we grant, which we possibly need not, that all the evidence really is in support of the law, we can only be happy in such a concurrence if we have reason to believe that we can detect exceptions to it, if there are any. Mill's excursion into psychology here, then, may be perfectly sound as psychology. But it does nothing to establish the doctrine that the law of universal causality is an induction from, and is true of, the external world. Instead, this doctrine becomes merely a recapitulation of Hume's argument that a knowledge of necessary connection is not based upon impressions of external sense.

For Mill the minimum requirement for scientific knowledge is that we should be able to determine the laws of the facts of experience. But determining their laws does not involve the logician in a theory of the facts. The theory of the facts belongs to various other sciences, such as physics, psychology, and metaphysics. The logician is not interested, for example, in thinking as such, but in the products of thinking. "Logic is not the theory of Thought as Thought, but of valid thought; not of thinking, but of correct thinking."[230] There is an important distinction being made here between the laws of thought, with which psychology concerns itself, and the laws, in the sense of conditions, on which valid reasonings depend. It is a law of psychology that experiences are related by us in various ways. But it is a law of logic that <u>nota notae est nota</u>. It is, however, clear that in some sense or other Mill's <u>Logic</u> is dependent on psychology. And it is very important to make the nature of the dependence clear.

Even though the precept <u>nota notae est nota</u> may be an induction, it is an induction from valid thinking, not just from instances of thinking selected at random. Mill has, then, either to demonstrate that we have some direct way of recognizing valid thinking, or he has to make his precept prescriptive rather than descriptive.[231] Of these two alternatives, he prefers the former. Valid thinking is that which can be corroborated by intuition, if not in fact, at least in principle. And this is the sense, as it seems to me, in which logic is dependent on psychology for Mill:

> In no case can thinking be valid unless the concepts, judgments, and conclusions resulting from it are conformable to fact. And in no case can we satisfy ourselves that they are so, by looking merely at the relations of one part of the train of thought to another. We must ascend to the original sources, the presentations of experience, and examine the train of thought in its relation to these.[232]

To rely solely on introspection here involves the fallacy of supposing that whatever is in our minds now was there from the beginning. In Mill's view, in order to get at what is really intuitive it is necessary first to determine, and then eliminate, those convictions, beliefs, and supposed intuitions, which can be accounted for in terms of the laws of association. It is only the original elements of consciousness which are intuitively certain and which can function as a criterion to which Mill's view of truth will conform. For he holds that a statement is true when it conforms to the facts. But the facts are not identical with the present deliverances of consciousness. (If they were, Copernicus could hardly have triumphed over Ptolemy.) Since, therefore, we cannot rely on introspection to tell us what portion of our experience is intuitive, some other method of arriving at this portion of experience is necessary. It is here that psychology renders to logic a necessary service. Just as ethics gives us rules or precepts which, if followed, enable us to attain a given end, but is dependent upon some inquiry other than itself for the determination of what, in fact, the desired goal is, so logic gives us rules for correct thinking, but is dependent upon something other than itself for examples of correct thoughts.

Logic presupposes that we know certain statements to be true, and the problem for logic is the extension of knowledge, by providing canons which enable us to judge the inferences which we make, and the hypotheses which we formulate. Most of our speech concerns objects, which are inferences originating, temporally speaking, in sensations. Following such inferences sensations appear within perception as attributes of objects. Some attributes are said to be essential, others accidental. Definitions are formulated in terms of the former. The word "man" denotes all men, and connotes the essential attributes of all men. The truth of the statement, "All men are mortal," can only be determined by an empirical inquiry as to whether or not humanity is a mark of mortality. (For Mill it cannot be determined within the context of language alone.) And to what can we appeal but the original data of consciousness--from which Socrates, or the Duke of Wellington, or any other man, is an inference--to determine whether or not that constant cluster of sensations from which we infer the presence of a man, are marks of mortality? And to what science but psychology can we turn to find out what those original sensations are?

Psychology cannot, however, tell us that all men are mortal. It can only tell us that we believe all men are mortal, and why we believe it. In telling us why we believe it psychology performs the valuable function of analyzing the content of consciousness. (And Mill tells us that a man's synthesis can only be as good as his analysis.) Psychology,

then, gives us the elements of thought--*e.g.*, sensations and inferences--but to provide tests for the validity of thought lies outside its province. The logician, in providing canons of inference, deals with the same elements as does the psychologist. But he deals with them differently. The logician does not ask why we believe all men are mortal, but asks, Is the proposition 'All men are mortal' true? If so, what are the grounds, not for the appearance of the proposition within my consciousness, but, of its truth? The grounds of its truth are the relations in which it stands to the *incontrovertible* assertions of consciousness. And only psychology can tell the logician what these incontrovertible assertions are.

Psychology, then, performs at least two important functions from the standpoint of the logician. First, it isolates that element of knowledge which is intuitive. And, secondly it thereby at least indicates the criterion of truth. A statement is true when it has and can be known to have, the quality possessed by that knowledge which is intuitive.

It should also be pointed out that Mill does not always turn over to the psychologist the all important task of analysis. Instead, as in the *Examination of Hamilton*, Mill frequently undertakes this task himself. And in the *Logic* he frequently turns the task over to the "metaphysicians":

> Of the science . . . which expounds the operations of the human understanding in the pursuit of truth, one essential part is the inquiry: What are the facts

> which are the objects of intuition or consciousness? . . . But this inquiry has never been considered a portion of logic. Its place is in another and a perfectly distinct department of science, to which the name metaphysics more particularly belongs: that portion of mental philosophy which attempts to determine what part of the furniture of the mind belongs to it originally, and what part is constructed out of materials furnished to it from without.[233]

3. Theory of Knowledge

In order to devise a "formula" which will state Mill's theory of knowledge, it is necessary to keep the following kinds of knowledge in mind: (1) immediate knowledge of conscious states, (2) inferential knowledge. I think a single formula will suffice here: Knowledge is the relating of particulars.

It may seem that the formula does not summarize satisfactorily what Mill says about our immediate knowledge of our conscious states. It may be asked, What "relating" takes place here? It may be pointed out that conscious states are not known as related, but as unrelated. My answer to this possible objection is that, for Mill, conscious states are known as related to consciousness. They are also known by the mediating agency of memory.[234] I know that a sensation, for example, is present to consciousness. Or I remember that I had a similar sensation yesterday. Sensations do not know anything. And, therefore, knowledge cannot be defined in terms of sensations alone.

In themselves, considered from the standpoint of their "existence," sensations may be unrelated. But existence is

something we infer. It is not an item of immediate knowledge. That inferential knowledge consists in the relating of particulars, for Mill, is too obvious to merit exposition.

4. Metatheory of Knowledge

I call Mill's attempt to defend the above theory of knowledge his metatheory of knowledge. What defence does Mill offer of the formula--that it *is* his formula is, of course, a claim of mine, not his--that knowledge consists in relating particulars? Mill's defence here is that the truth of the above formula as a descriptive statement, is intuitively discerned. In order to justify the formula as a prescriptive statement, a metatheory of knowledge is required.

The most important claims in Mill's metatheory are as follows. (*a*) There are particulars. (*b*) There are methods of relating particulars. (*c*) There are particulars of a specific kind, namely, relations, which relate particulars.

That propositions (*a*), (*b*), and (*c*) above are true as descriptive statements is intuitively evident. In order to justify that they are necessary prescriptions, a meta-metatheory is required.

It is hard to be certain about Mill's meta-metatheory. It is my opinion that he has two meta-metatheories, and that he appeals, at any given time, to whichever best meets the demands of the moment.

One meta-metatheory asserts that there are substances.

Substances are of two kinds, subjects and objects. And there are relations. Objects and relations accomplish the mediation of particulars from the standpoint of that which is ontologically real. Subjects, and the dispositional powers of subjects, accomplish the mediation of particulars from the standpoint of knowledge.[235]

The other meta-metatheory asserts that there really are only subjects, the dispositional powers of subjects, and the content of the consciousness of subjects.[236] All else, including objects and the knowledge of objects consists of inferences from these.

As descriptive statements, the above meta-metatheories are intuitively evident. As prescriptive statements their validity--i.e., their necessity--has to be established in a meta-meta-metatheory. My view is that here we find the single statement, Whatever is intuited as valid is valid--("Valid" may mean either "true" or "necessary," depending upon whether it is predicated of a descriptive or of a prescriptive statement.)

But one could demand support for the statement that whatever is intuited as valid is valid. In order to satisfy this demand a meta-meta-meta-metatheory would be required. And so on, ad infinitum.

CHAPTER VIII

CONCLUSIONS

A. Preamble

In this chapter I will not recapitulate unnecessarily the material already presented. Instead, having presented the epistemologies of Mill and Whewell, I now wish to set forth a number of conclusions which, in my view, the material already presented supports. And, since it is my thesis that the epistemological views of Mill and Whewell are essentially in agreement, I shall devote this chapter primarily to pointing out points of agreement. In general, my argument will be that since Mill and Whewell agree as to what knowledge is, agree as to how it is obtained, and agree as to how we know when we possess it, any disagreements they have concerning knowledge cannot be too serious. When I say that they are essentially in agreement I do not mean that they are completely in agreement. For they obviously are not completely in agreement. Instead, I mean that they agree on certain fundamental issues such as the three cited above: (1) What is knowledge? (2) How do we obtain it? And (3) How do we know when we possess it? There are eleven points of agreement which I claim to have established, and which I state as my conclusions. These are listed at the end of this chapter.

Some of my conclusions are not likely to be disputed. But others may seem questionable. Therefore, before I state them I wish to make a few preliminary comments.

My conclusions assert that the two epistemologies are very much alike. There are three ways in which one could attempt to show that they are alike. (1) One could take some independent set of standards and compare Mill and Whewell with these. (2) One could take Mill as a standard and compare Whewell with him. Or (3) one could take Whewell as a standard and compare Mill with him. Although I have employed all three of the above to some extent, for the most part I have employed the third. And when, in my list of conclusions, I point out that the two positions are very similar, I am implicitly asserting that Mill's position is very much like Whewell's.

At first glance, Mill's position is not at all like Whewell's. And, in order to justify the conclusions set forth at the end of this chapter, it is necessary to show that this "first glance" is not trustworthy. To show that it is not, I wish to recapitulate a few of the main points which I have already established and to discuss some problems associated with Mill's views. I do not contend that as a result of this discussion I can show that Mill always agrees with Whewell. But I contend that I can show that on certain fundamental issues he does agree, and that on certain others he cannot very well disagree.

For Whewell the method of obtaining knowledge is essentially the method of the inductive sciences. Whewell tries

to group together induction and deduction in such a way that they virtually constitute a single method. In the main--though there are exceptions to this--induction is the method whereby new knowledge is acquired. By induction is meant the colligation of facts. The new truths acquired by men like Kepler and Newton provide examples of inductions.

But when we come to set our knowledge down in such a way as to make it clear that it really _is_ knowledge, the method of deduction is to be preferred. This view explains Whewell's writing the _Mechanical Euclid_ in which the laws of mechanics and hydrostatics are set forth, not as colligations of facts, but as following necessarily from axioms and definitions. The advantage of such a work, Whewell says, is that it gives to the principles of mechanics and hydrostatics "rigorous proof" of the kind found in Euclid. So I should say that for Whewell induction is the method whereby new knowledge of the physical world is discovered, but deduction is the method of proof. And it can be shown that intuition is what supports each method.

Induction, or the colligation of facts, involves three processes: the explication of conceptions, the recognition of facts as facts, and the recognition that some conception "fits" the facts. The recognition of facts as facts is intuitive. Facts are either intuitively discerned to be facts, or they are not discerned at all. The colligation of the facts also requires in the end, an act of intuition. Even though it may be preceded by numerous attempts of the trial

and error variety, the appropriate conception is finally recognized intuitively. It is not, admittedly, recognized as such in one flash of insight, but by a great many intuitively seen relationships as, for example, between the geometrical conception of an ellipse and the successive points in space occupied by a planet. In deduction it is even more obvious that we rely on intuition every step of the way. The conceptions are derived intuitively from--are "unfolded out of"--intuitively known Ideas. Furthermore, as Descartes says in the *Regulae*, in our dealings with the external world it is very easy to make mistakes. But where deduction is concerned, any intellect, even minimally rational, cannot make mistakes provided the steps in the argument be kept sufficiently small. The reason then for writing such a book as the *Mechanical Euclid* might be said to be to give to mechanics a certainty equal to that of the demonstrations of arithmetic and geometry. And the reason geometry and arithmetic possess this certainty is that every step in them is intuitively certain.

Hence I think we must suppose Whewell felt that although intuition ultimately guarantees every valid induction, and makes it possible for "theory" to be regarded as "fact," there is a stage in which we are necessarily dependent upon deduction if we are to accept the induction. He recognized that the colligation of facts by means of a conception of the intellect had something hypothetical about it. And when he claims that ultimately the colligation is capable of intuitive verification, I think we must hold that he means something

different by "intuition" here than he does when talking about geometry as a strictly deductive science. The verification of the colligation by direct intuition of fact is achieved when one intuits the colligation as a fact, as opposed to thinking it as a theory. Deduction, on the other hand, proves the theory true qua theory by showing that each step in the proof is intuitively known to follow necessarily.

Whewell's methodology, then, includes an infallible method, the method of deduction. But that it is infallible is demonstrable only on the level of metatheory, where it is shown that its infallibility is grounded in the infallibility of intuition. Its actual employment as an infallible method simply presupposes, but does not attempt to prove, except by an appeal to the universal consent of those who use it, that it is infallible. Leaving aside any ontological considerations, the certainty of deduction can be traced to the tautological statement that the mind knows intuitively the forms of knowledge. In Whewell these forms take on the existential status of Ideas. On this point we need not agree with him. But that the mind knows the forms of knowledge is, I should think, incontestable. The major difference which I see between a contemporary approach and Whewell's on this level is that he tends to think of the forms of knowledge in terms of forms of perception whereas modern writers tend to emphasize linguistic forms. This provides a distinction which I introduced earlier between knowledge as a process and knowledge as the result of a process. Whewell emphasizes the

former, contemporary writers emphasize the latter. Hence in the modern age we hear a great deal of talk about talk. In Whewell we have a great deal of talk, historical and analytical, about thought.[227]

Mill is like Whewell in that he sees as part of the philosopher's problem the analysis of valid thinking, and he is like a contemporary writer in the extent to which he recognizes that the forms of knowledge are just as much linguistic as they are mental and perceptual. And he sees that there are universal and necessary laws of thought. In his _Examination of Hamilton_ there is an explicit account of his position on this point.[228]

An apparent point of disagreement between Mill and Whewell arises here. But, upon examination, this turns out to be a purely verbal disagreement. Whewell holds that much of our knowledge, and, especially, much of our knowledge of the forms of knowledge, is _a priori_. Mill frequently mentions the term _a priori_ in his writings and asserts that he will have nothing to do with the conception which the term represents. This attitude of Mill's is based upon a misinterpretation--or what I hold to be a misinterpretation--of Kant's usage of the term. I have not discovered any place where Mill explicitly states what the term means for him. But his usage indicates that he would accept the definition offered by Sir William Hamilton:

> The term _a priori_, by the influence of Kant and his school, is now very generally employed to characterize those elements of knowledge which are not obtained _a posteriori_,--are not evolved out of experience as factitious generalizations; but which, as native to,

> are potentially in the mind antecedent to the act of experience, on occasion of which (as constituting its subjective conditions) they are first actually elicited into consciousness.[239]

In the sense defined above, Mill would have nothing to do with the *a priori*. And, to the best of my knowledge, neither would Kant. If we mean by *a priori*, as Kant consistently seems to have meant by it, "universal and necessary," then Mill would agree with Whewell that knowledge contains a universal and necessary element.

Mill will not say that the laws of thought currently accepted are the only such laws possible. That is to say, he will not commit himself as to whether other laws may or may not hold in other times and at other places. But one point he states emphatically is that some laws of thought are real necessities to a given person at a given time. The three Aristotelian laws, he goes on, are possibly not absolute in the sense that they determine the only form which thinking can take. But to the extent that they hold for the thinking of any given individual at any given time, they hold absolutely. Mill's argument here is not that the laws of thought determine our experience, but that our experience is of such a nature that from it we may infer that the laws of thought are absolute in relation to the kind of experience which we have and which we anticipate. The only reason we could have for abandoning the present laws of thought in favour of others would be if our experience were to change. But as long as our experience does not change, the validity of the laws of thought is inescapable:

> I readily admit that these three general propositions are universally true of all phaenomena. I also admit that if there are any inherent necessities of thought, these are such . . . . They may or may not be capable of alteration by experience, but the conditions of our existence deny to us the experience which would be required to alter them. Any assertion, therefore, which conflicts with one of these laws . . . though it were on a subject wholly removed from the sphere of our experience, is to us unbelievable. The belief in such a proposition is, in the present constitution of nature, impossible as a mental fact.240

I now wish to point out what seem to me four weaknesses in Mill's account of the laws of thought. (1) I am willing to grant that for any given inquirer the laws of thought may be, as far as his own mental history is concerned, known inductively. But I think it is impossible to establish inductively whether the laws of thought are descriptive or prescriptive of nature. Whatever is the truth here has to be established at some other level of discourse than induction. Furthermore, Mill should make a sharper distinction between what he calls in the quotation above the "constitution of nature," and "experience." It is from his experience of nature, not from nature, that he has derived the laws of thought. And he has no way of knowing, simply by the employment of induction, whether or not nature as it is experienced is the result of its being ordered by the observer employing the laws of thought in order that nature may be intelligible, or whether nature exhibits a preexistent order which experience in some mysterious and fortuitous way duplicates. Such a problem is one for metatheory to decide, as Whewell sees very clearly. And, as I have indicated in the central chapters of my thesis,

there is a great deal of evidence to suggest that for both Mill and Whewell experience is as it is because of the order which we introduce into it.

(2) Mill asserts that all that is undeniably given are feelings, that is to say, sensations, emotions, and volitions. In some mysterious way ideas are added to these "original" data. Ideas are "mental representations" of the original data. An equally mysterious activity contributes objects. Given feelings, ideas, and objects we, by the employment of certain stated methods, discover the laws of nature and the precepts of art. As far as establishing the validity of the above claims is concerned we ultimately have to rely solely on intuition. But as to how we know that our intuitions are reliable and constitute proof, Mill has little to say. Now, this certainly does not prove that on the level of metatheory he would agree with Whewell that our intuitions are true for the reason that they simply represent the discovery of the orderliness which mind, either human or divine, introduces into the essentially nonrational world of sensation and feeling. Nevertheless I think it can be shown either that he does implicitly agree, or at least that he cannot very well disagree.

What it means to be rational, Whewell says, and I think Mill would concur, is to be known in accordance with a rule or principle. Sensations per se, taken strictly in themselves and of themselves, have no rational characteristics. They simply are. We possess knowledge, not when we know what phenomena there are, but when we know the laws of the phenomena.

And while Mill insists that the laws are discovered, not introduced by the mind, it is necessary to emphasize that this holds only on the level of induction. And on this level what he is saying is undoubtedly true. But the cognitive significance of the laws depends upon prior determinations which the inductive level cannot justify as, for example, that the cognition of temporal relations of phenomena is what is meant by knowledge. Induction can discover what the temporal relations are. But it cannot discover their significance.

(3) A major difficulty in Mill is to some extent circumvented by an appeal to that particular kind of mental activity which yields knowledge of objects as such. By the employment of the name of this activity--"inference"--Mill introduces into his object language something which, I think, does not properly belong there. Although "object" may properly be a term which appears within the language of induction--i.e., within Mill's object language--the term "inference," when it refers to the origin of objects as data of knowledge does not really belong within that language. If we are to have a theory of objects it will properly have to be a metatheory. Mill claims that objects result from a mental activity which we have performed but which we cannot recall. If we claim, as Mill does, that we have this knowledge of the object over and above the laws of its behaviour, this cannot be the same sort of knowledge as the laws of its behaviour. "Inference," then when it refers to our knowledge of the origin of objects does not belong to the same language as "objects" and "induc-

tion." "Inference," here, does not refer to something of which we have first order proof. Instead, it refers to a condition of the possibility of the appearance of objects within perception. Mill and Whewell, therefore, agree that the appearance of objects within perception--i.e, as data of induction--is the result of the activity of the knowing subject. And they further agree that to establish that the appearance of objects within perception is the result of the activity of the knowing subject is not established by an examination of objects per se.

(4) Our knowledge of the laws of objects results, according to Whewell, from the colligation of facts, and according to Mill from the employment of the methods of experimental inquiry. In order to defend his position Whewell introduces a complex metatheory. Mill, in the Logic at any rate, attempts to dispense with a metatheory and still justify his position. I am inclined to agree that to some extent he succeeds. If it be asked, for example, What is the proper method of inductive inquiry? he gives a pretty defensible answer, couched in object language terms. However, there are difficulties here. One is that the employment of the methods which he advocates requires intuition. And a second is that it requires memory. And Mill's object language is not equipped to deal with these. His object language is equipped to deal with objects which are perceived as objects, within space and time. But intuition is not a property or quality of an object. It is, instead, an act of a subject.

However, he does suggest a way out of this difficulty by maintaining that all that can be thought in a pure description of a sensed datum is that it just is. It is not necessary to say that it is intuited. But when we begin to name things, difficulty arises, because we cannot name without classification. And classification involves more than an assertion that something simply is. It requires in addition to this an account of predication. If we deal only with the relations of units of discourse to one another there are ways of getting around these difficulties. But where the relation of names to the things which they name is concerned Mill begins to feel obliged to supply not only a description of predication and its rules but also a justification of it. And here he does not succeed in staying within his object language. I have already reviewed this material but shall just repeat one example. Among the kinds of things that can be named are subjects. And these are not objects. Memory, the second problem referred to a few lines above, Mill simply calls a mystery. I think then that Mill's theory of knowledge is dependent for its justification upon a metatheory in which intuition and memory are shown to be among the supports of the theory, not constitutive elements of the theory.

It seems to me, then, that Mill's theory of knowledge has essentially the same support that Whewell's has. The following points are illustrative of this. First, by "induction," each means the bringing together under some mentally comprehensible form--concept, class, rule, law, and the like--the

basic elements of consciousness which in themselves are viewed as noncognitive--or, at any rate, as precognitive--and if not exactly irrational, at any rate, prerational. To experience the data of induction is not, properly speaking, to know the data. Secondly, each agrees that knowledge is a product of the mind, not of the sense-organs. Mill and Whewell are both mentalists to this extent at least. Thirdly, we find both men agreeing that the mind in knowing knows something other than itself. Mill and Whewell are both realists in this respect. I have argued that on the level of induction itself this realism can only be asserted or presupposed. Induction cannot prove the independent reality of the world of objects. This proof can only be supplied by a metatheory of induction. And both Mill and Whewell tend to agree that the world which is known to be objectively real is a product of the acts which constitute thinking about experience. Fourthly, at the same time that the independence of the external world is asserted it is to some extent denied. The external world *per se*, if there be such a thing, would be independently real possessing its own unknown characteristics. But the known external world, while spatially external to us, has an existence within our cognition of it. Within this cognition both the external world and we ourselves, as objects, appear. To the knowing mind we, as objects, appear as other than the world. But within such a context we can have the same knowledge of ourselves as we have of any other object. Fifthly, both agree that to know ourselves as knowers--that is to say, as knowing truth

from falsehood--we have to think of ourselves in some other terms than object terms. We have to know ourselves as the source of the forms of the knowledge of objects. We are now asking not, What do we know, and under what forms do we know it? but, Whence do the forms of knowledge originate? Both Mill and Whewell seem to me to give the same answer here: They are derived analytically from experience. But they are justified intuitively.

Yet a knowledge of the forms of knowledge is not enough. The knowledge we generally want is a knowledge which incorporates the forms with some empirical content. And while it is believable that the knowledge of the forms may be intuitive, it is not so obvious how we are to justify our empirical knowledge. At first sight, for Mill it seems to be the case that we know that our statements about experience are true because in language we are simply describing experience. To say "The sky is blue" is simply a complex way of pointing to the blue sky. But the situation is more complex than Mill acknowledges. There is, for example, the problem of distinguishing between "The sky is blue" and "'The sky is blue' is true." Mill would feel that each of these propositions simply asserts that if one were placed in such and such circumstances one would have such and such experiences. However, there is more than this involved. From the standpoint of the model of the knowing situation we can picture to ourselves the statement "The sky is blue" as involving a perceiver and what he perceives. But what is the model of the knowing situation in

relation to the statement "'The sky is blue' is true?" Mill's orientation requires that the data of the inference, "The sky is blue," also constitute the evidence which proves the truth of the proposition.[241]

There are, however, problems here which Mill is ignoring. In the first place, to ask for proof seems to me a different sort of question than to ask for data. If, as Mill holds, the data also constitute proof, then the question, What is the proof of any given inductively arrived at proposition? is nonsensical. If the data and the proof were one and the same, no proof would be needed, or asked for. But proof is requested. And this request for proof seems to me to involve a non-empirical epistemological subject, which asks for reasons rather than for data. It not only asks: "How do you know that the sky is blue?," but it also asks: "Why do you point to the sky in answering my question, 'How do you know that the sky is blue?'?" There is a decision here which establishes that sensible data constitute proof. The data themselves do not carry this guarantee with them.

To return to the example, "The sky is blue," I think it is clear that such a proposition involves more than a verbal pointing to sensory experience. All that can be asserted on the basis of purely sensory experience here, I believe, is, "Blue here-now." Therefore, to attempt to explain what we mean when we say that the sky is blue it is not sufficient to point heavenwards. Some sceptic could insist that he can see the blue here-now but not the sky and that, therefore,

the proposition "The sky is blue" and the word, "blue," are synonymous expressions. And it is clear that they are not intended to be. Mill, of course, would grant all this, although he does not use this particular example. Blue is a sensation, the sky is an inference from sensation. There are two levels of language involved here, and also two levels of knowing. There is the level of immediate awareness, and there is the level of intelligible discourse about inferences. But, having admitted this distinction, Mill is unable to deal satisfactorily with it. On the level of simple awareness a fairly defensible case may be made out for the contention that the data are also proof. But they are only proof of meaning. By pointing to the sky we can indicate what "sky blue" means. But that "sky blue" is the name of that colour is determined by an intellectual and arbitrary act. And in the case of the proposition, "The sky is blue," pointing to the sky cannot even establish its meaning--unless we equate "The sky is blue" with "blue"--let alone prove that the statement is true.

To say that we know that "The sky is blue" is true seems to get us into an even more complicated situation unless it be denied that there is any difference in meaning, and in what is being asserted as true, between "'The sky is blue' is true," and, "I know that 'The sky is blue' is true." Mill would tend to argue that these statements are simply extensions of the most primitive description of experience which we can allow to be possible. His argument would be as follows: I know that "The sky is blue" is true, because I know that the sky is

blue. And I know that the sky is blue because, in the first place, this is a description of my experience and, secondly, the proposition is constantly reaffirmed by experience. Now, I feel that there is a difficulty here, but I am not sure that I quite know how to point it out. Perhaps with the aid of a couple of diagrams I can make clear what I have in mind.

In the first place, I contend that I have shown that Mill accepts the subject-object dichotomy. The model of the knowing situation, therefore, for the statement, "The sky is blue," would be

$$\text{subject}_1\text{--object}_1 .$$

If the object were simply the sensation, blue, the dichotomy might be avoided. However, the object is not the sensation blue, but the blue sky. To say that the only thing that is "really" "there" is the quality, blue, is beside the point. For what is seen is the blue sky, and this would not be the case without the activity of the subject which infers the sky from the sensed data. The proposition may be a valid report of experience. But although it seems to be merely a description by the subject of an object, it is more than this. For some process has gone on here as a result of which "the sky" is a perfectly meaningful term which means something other than "blue." As pointed out above, "The sky is blue" is not the same proposition as "Blue is blue." The former is synthetic, the latter analytic.

The next question is, How shall we represent in a diagram, the proposition "I know that 'The sky is blue' is true?"

The model is still essentially subject-object, but the object has become more complicated in that it now includes what was labeled above, "$subject_1$", and might be diagrammed as follows:

$$subject_2\text{--}(subject_1\text{-}object_1).$$

"$Subject_2$" above, refers to the "I" in "I know." And "$subject_1$-$object_1$", above, refers to the experience which is reported as "The sky is blue." A return to experience (but not to sensation) may establish what the proposition "The sky is blue" means. But I contend that a return to experience cannot prove that the proposition is true. In order to get at the truth of the proposition we have to introduce a certain amount of intellectual curiosity and also a certain amount of talk about talk. For example, we shall ask, What is the sky? Which of Mill's categories--feeling, substance, attribute--does it come under? And we shall also be interested in the relation here between the logical subject and predicate. Is it the case that there is an x (the sky) and that of it being blue can be predicated? Now, whatever the right answers are here, it seems that they are not to be derived from a mere return to experience. What is required instead is insight into experience and into the forms of knowledge which characterize propositions arising out of the original knowing situation.

From the above material I conclude that Mill's metatheory, whatever it may be, cannot be equated with the associationistic psychology of the book on Hamilton. The associationistic psychology <u>may</u> be the basis, or the metatheory, of the first level of knowledge, but not of any higher level. Instead

what is required, as all the major figures in the western philosophical tradition seem to have recognized, is the introduction of the self awareness of the knowing subject and of its knowing activities.

I suggest, therefore, that as members within the same philosophical tradition Mill and Whewell are more understandable, and their philosophies more significant, in terms of their likenesses than in terms of their differences. There is a knowledge which employs observed particulars but which is not knowledge of particulars in the way in which direct experience is knowledge of particulars. I have agreed with Mill that on the level of the person actually involved in experience, a statement to the effect that the only things observable are particulars may be descriptive of that experience. But I contend, with Whewell, that such a statement is prescriptive for a knowledge of that cognitive experience when the experience and not the particulars observed within experience becomes the object to be known. And I further contend that on this fundamental issue Mill and Whewell necessarily agree.

B. Conclusions

With the above by way of preamble, and presupposing the central chapters of the thesis as evidence, I contend that the epistemologies of Mill and Whewell are in fundamental agreement on the following highly important issues.

1. They agree concerning their undertaking. Their epistemological systems are directed towards the same end.

This end is to state the grounds of the possibility of knowledge.

2. Although it is very difficult to define "knowledge" in a generic sense, I shall suggest two definitions which I find unobjectionable. I am inclined to believe that Mill and Whewell would subscribe to each. I shall, therefore, say that the meaning of knowledge in its generic sense is the second major point on which they agree. The first definition I shall offer is that "knowledge" means "true statements known to be true of experience, actual and possible." The second is that "knowledge" means "the relating of particulars."

The two definitions are very closely linked. In order to understand the former it is necessary to append the latter. For, "experience, actual and possible," referred to in the first definition, is always of particulars. But it is not of particulars *per se*. Instead, it is of related particulars. I contend that Mill and Whewell would agree that the two definitions, taken together, are satisfactory because (*a*) this is something to which everyone will subscribe; (*b*) it is constantly confirmed by the practice of the recognized sciences; and (*c*) it is capable of support on the level of metatheory.

When we examine the support which can be provided for them on the level of metatheory we discover that they have to be supported as true descriptions and as prescriptions. As descriptive statements--*i.e.*, as statements describing the conclusions of the recognized sciences--they have the support of intuition. But we can always ask what support intuition

has. As a prescription, the statement, "Knowledge consists in the relating of particulars," can be acted on as if it were true. But one cannot really prove a prescription to be true or false. The most one can do is prove it to be universal and necessary. And in order to prove that it is universal and necessary we have to appeal to intuition. In this case we claim to intuit a universal and necessary form of knowledge.

3. The third major point of agreement, then, between Mill and Whewell, is that the ultimate ground of knowledge is intuition. But it is a meaningful question to ask, What supports intuition? Neither Mill nor Whewell really "gets to the bottom" of this problem. Instead, they carry their analyses of the supporting grounds of knowledge only so far. Having gone this far they then take a stand. They make what I called earlier a "decision." They decide to accept the evidence of intuition as coercive. They decide to act as if the declarations of intuitions were incontrovertible. They do not first prove--and, indeed, no one can, since the number of metatheories is theoretically infinite--that the declarations of intuition are incontrovertible. In order to act, one has to call a halt to the search for "first" principles. Mill and Whewell call a halt when they arrive at intuition. The scientist, qua scientist, calls a halt much earlier. He decides to act as if there were particulars and as if knowledge consisted in relating them.

4. Because of their particular orientation to the nature of knowledge, the respective analyses of knowledge conducted

by Mill and Whewell focus on particulars and their relations. Knowledge is about particulars. And particulars are "particular" in two senses. (a) Each particular is distinguishable from every other. And (b) each particular is distinguishable from that which knows it as a particular. Out of this orientation arise the orders of knowledge, and the theory, and meta-theories, of knowledge, to which Mill and Whewell subscribe. The fourth basic point on which they agree, then, is the necessity of realism.

5. The fifth essential point of agreement between them is that a correspondence theory of truth is the theory to be preferred. Although they disagree as to the existential status of particulars, and although they disagree as to the manner of their relation, they agree that knowledge is the relating of particulers. And they agree that in a statement we assert the manner in which particulars are related. They also agree that a statement is true if, in fact, particulars are related in the manner in which they are asserted to be related. (Error, of course, also consists in the relating of particulars. But this does not affect the validity of the assertion that knowledge consists in the relating of particulars. Nor does it affect the validity of the assertion that in a true statement particulars have the same relation in the statement that they have in reality.)

6. Concomitant with their subscription to realism, and to a correspondence theory of truth, is their subscription to the spectator model of the knowing situation.

7. The seventh important point on which they agree is that there are three basic methods of knowing: intuition, induction, and deduction.

8. They further agree that the kinds of knowledge are related, and that it is possible to construct a classification of the sciences which makes clear the manner of their interrelation.[242]

9. They agree that the content of knowledge differs from the form, and that the content is arbitrary and illogical, whereas it is the form which the mind knows best. They also agree that although the form is possibly capable of being empirically discovered, once discovered it is seen to be prescriptive as well as descriptive.

10. Each agrees that the role of the knower is complex, and highly significant, and has to be established by a unique type of inquiry (metatheory). But Whewell is more conscious than is Mill of a necessary distinction between a knower which can be investigated by psychology, and a knower which is germane to epistemology--*i.e.*, a knower who can distinguish truth from falsehood.

11. Each agrees that knowledge is not entirely explicable. This inexplicability arises in part because no reason can be given for the nature of its sensuous content. And it arises in part because there are always statements which we "know" to be true, but which we cannot prove to be true. These statements can only be incorporated within knowledge by an appeal to an intuitive knowledge of those truths which are basic to all others.

Since Mill and Whewell can agree on these eleven items, and since these items are basic to any epistemological system, I contend that their theories of knowledge are not so far apart as they "at first glance" appear to be. Furthermore, if we regard Whewell as a Kantian and Mill as a positivist, then we may generalize upon the preceding observation. To the extent that Whewell is a Kantian and Mill a positivist, Kantian and positivistic theories of knowledge are not so far apart as is sometimes supposed. In fact, if Whewell and Mill are typical representatives of the "isms" suggested, then I contend that to distinguish Kantians from positivists, qua epistemologists, is not unlike distinguishing animals from quadrupeds, horses, asses, and ponies.

NOTES

1 Ledger Wood, "Epistemology." Dagobert D. Runes, *The Dictionary of Philosophy* (New York, no date), 94.

2 William Whewell, *The Philosophy of the Inductive Sciences* (London, 1847), Volume 1, Book 1, Chapter One, page 1. Because of a peculiarity of pagination, it is difficult to refer to pages 1-18 of this book. There is a section which precedes Part I and which has pages numbered 1-18. In Whewell's Table of Contents this introductory material is designated "Preface." But in the text itself it is designated "A Letter." This introductory material has pages numbered 1-18. Then, beginning with Book I, the pages are renumbered, beginning with 1. I wish to make one more reference to the introductory material. (See note 20, below.) And I shall designate it "Preface." In all other references to this book, if the page number is less than 19 the reference will be to Part I, not to the preceding material.

3 See, *e.g.* C.D. Broad, "The Local Historical Account of Contemporary Cambridge Philosophy." C.A. Mace (ed.). *British Philosophy in the Mid-Century* (London, 1957), 58.

4 I am relying on Copi here. "In any investigation of language, there is an object language which is the *object* of investigation, and there is a metalanguage which is *used* by the investigators in talking about the object language . . . . Object language and metalanguage are *relative terms*. Any language . . . is an object language when it is being talked *about*." Irving M. Copi, *Symbolic Logic* (New York, 1954), 186-187.

5 Morton White, *Toward Reunion in Philosophy* (Cambridge, 1956), 288.

6 See C.J. Ducasse, *Philosophy as a Science* (New York, 1941), 174, 238.

7 John Stuart Mill, *A System of Logic, Ratiocinative and Inductive, Being a Connected View of the Principles of Evidence, and the Methods of Scientific Investigation* (London, 1862), Volume I, 291. This is the fifth edition of the *Logic*. All references to the *Logic* will be to this edition unless

otherwise indicated. See also William Whewell, <u>On the Philosophy of Discovery</u> (London, 1860), 249-250.

[8] Mill, <u>Logic</u>, I, 291.

[9] <u>Ibid</u>.

[10] James Martineau, <u>Essays Philosophical and Theological</u> (New York, 1868), I, 73. (The reader will note that Martineau and I have our spatial metaphors reversed.)

[11] <u>Ibid</u>., 74.

[12] Whewell clearly holds that there is such a science, even though he does not say a great deal about it. Time, for him, is a form of perception and of intuition. There is a factual science of time, and a pure science of time. The factual science deals with that which is perceived under the form of time. Here the sidereal day functions as a unit of measurement. The pure science of time has the Idea of time as its subject matter. The name of this science is theoretical arithmetic--"the speculative doctrine of the properties and relations of numbers." Whewell relates the two sciences of time by introducing the terms "rhythm" and "repetition." Rhythm signifies "the recurrence of times similarly marked." From "the repetition which time admits of" we "collect" the conception of number. The best summary of Whewell's views concerning time is found in <u>Philosophy of the Inductive Sciences</u>, I, 128-132.

[13] See Whewell, <u>Philosophy of the Inductive Sciences</u>, I, 18.

[14] <u>Ibid</u>.

[15] <u>Ibid</u>.

[16] <u>Ibid</u>., II, 651.

[17] Whewell, <u>Of Induction</u>, 77.

[18] See Whewell, <u>Of Induction</u> 78, and <u>Philosophy of Discovery</u>, 292-299.

[19] I should like to say one or two things in support of Whewell's ethical position. The content of his ethics I cannot entirely accept. Nor can I agree with him that his ethical theory has the support of divine revelation. But where his methodology, or what he would call his philosophy, of ethics is concerned, I believe his arguments to be essentially valid.

I agree, for example, that ethics has first of all to be distinguished from psychology and anthropology, for ethics has the peculiarity of being prescriptive. And although I agree with Schlick that "ought" means no more than "somebody wants me to do something," with the *proviso* that one of those somebody's may be myself, this definition of "ought" does not, I think, turn ethics into an empirical science. For the "ought" can still be interpreted in either of two ways, namely, as the demand of emotions, appetites, and affectivities, or as the demand of reason. As I see it, the mores result from the former, a moral code from the latter. That the two are distinct is clear. A man may very well violate his moral code in adhering to the mores. And the reverse is equally the case. Furthermore, although there is some individual variation where the moral code is concerned, there is pretty general agreement within any group as to what *the* moral code is. And the moral code is not established inductively. It is not established by going around asking people what the moral code is. A peculiar, and, I think, definitive feature, of a moral law is its absoluteness. Where a moral law holds, there is no room for freedom, and where there is room for freedom there can be no talk of morality. "Thou shalt not steal" as a moral law is absolute even though the mores may condone stealing. And the reverse side of the coin is that if one is free to steal or not to steal, it makes no sense to talk about the morality of stealing. Or, to take a further example, to eat meat on Friday is not for me a moral problem since I am free to eat meat on Friday or not to eat it. I agree, then, with Whewell that morality means action in accordance with a universal and necessary rule. And I further agree with him that these rules spring from reason, not from the appetites, emotions, and affections. For only reason can give to rules that universality and necessity. To the extent that men decide in conformity to reason they necessarily decide alike. To the extent that they decide in conformity to the other elements of their nature, they do not necessarily decide alike. As far as I can make out this is the only sensible explanation which can be offered to account for the difference which always exists between the mores of a society and its moral code.

[20] See Whewell, *Philosophy of the Inductive Sciences*, "Preface," 6. *Cf. ibid.*, 128, 317. See also, Whewell, *On the Philosophy of Discovery*, 325; Ducasse, *op. cit.*; 56-57, and M.R. Stoll, *Whewell's Philosophy of Induction* (Lancaster, 1929), 30.

[21] See Ducasse, *op. cit.*, 56-57.

[22] Whewell, *Philosophy of Discovery*, 307.

[23] *Ibid.*

[24] These claims are supported by a number of quotations which I have included in Chapters III and IV of the thesis. But I feel that some documentation should be given at this point. On the Philosophy of Discovery states clearly the appeal which Plato's views had for Whewell. At one point Whewell asks whether Plato or Aristotle had the truer view of nature and of science, and he decides that on this point "we must give the preference to Plato." Plato's view that the visible world is a "fleeting and changeable shadow" of a "real intelligible world" is, says Whewell, "extravagant." Nevertheless, Plato was right in seeking "the forms of the Intelligible Things." (See On the Philosophy of Discovery, 20.) Plato was also right in supposing "that the universe is framed upon the Divine Ideas; that man can to a certain extent obtain sight of these Ideas; and that when he does this, he knows concerning the universe." (Ibid., 357.) I do not see how, on the basis of such a view, man can properly be anything but an "observer."

But the position which Whewell usually defends is that man is an "interpreter" not merely an "observer." This I call the "Kantian" element in Whewell's philosophy. The propriety of calling it "Kantian" is indicated by passages such as Philosophy of the Inductive Sciences, II, 317-319. Here Whewell discusses Kant's "Copernican revolution" and concludes that Kant has succeeded in showing that "Ideas and Things, the Subjective and the Objective elements of our knowledge" are opposed, yet related elements, both elements being" equally real and equally indispensable." In the following passage there is an even more explicit acknowledgment of his indebtedness to Kant. The following quotation is from a paper written by Whewell on the occasion of the publication of Mansel's Prolegomena Logica. It is necessary to stress that Whewell wrote the following sentences. The reference in the quotation to "Dr. Whewell" is misleading, since it might suggest that someone else is the author. Such, however, is not the case. Whewell is the author: "Kant considers that Space and Time are conditions of perception, and hence, sources, of necessary and universal truth. Dr. Whewell agrees with Kant in placing in the mind certain sources of necessary truth; he calls these Fundamental Ideas, and reckons, besides Space and Time, others, as Cause, Likeness, Substance, and several more." (Ibid., I, 8.) Concerning these "conditions of perception" Whewell says (1) that we have pure sciences of them, and (2) that we employ them, and, especially, the Conceptions to which they give rise, to accomplish the colligation of facts. Where the pure sciences of the Ideas is concerned, man seems to remain an "observer." But in the colligation of facts he becomes an interpreter. The colligation of facts comes about when "some Conception is superinduced upon the Facts." (Ibid., II, 50.) "We may apply this term [--the Colligation of Facts--] to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which

are presented to our senses. The knowledge of such connexions, accumulated and systematized, is Science." (Ibid., 76) It is in determining how that which is given to the senses can best be related by that which is supplied by the mind that man becomes the interpreter of nature.

E.W. Strong, speaking of the Kantian and Platonic elements in Whewell's philosophy, says that Whewell "vacillates between a Kantian doctrine of a priori relations of thought, and a Christian Platonic doctrine of eternal, rational forms in the Mind of God . . . . Whewell's assertions about Ideas are . . . usually Kantian in character . . . ; but this greater frequency is no relinquishment of Platonic realism." E.W. Strong, "William Whewell and John Stuart Mill: Their Controversy about Scientific Knowledge," Journal of the History of Ideas XVI (1955), 212. (Cf. Whewell, On the Philosophy of Discovery, 359: "I do not conceive the doctrine . . . to be at all obsolete . . . that the Cause and Foundation of the Universe is a Divine Mind: and from that doctrine it necessarily follows, th t the laws of the Universe are in the Ideas of the Divine Mind.")

[25]There are two major discussions of the problems involved here. The first is in the Philosophy of the Inductive Sciences, I, 164-276. This is Book Three of the first volume, and is entitled, "The Philosophy of the Mechanical Sciences." The second is in Volume II of the same work, 578-594. This is an essay included in an appendix to the second volume and is entitled, "On the Nature of the Truth of the Laws of Motion." The ambiguity which words such as "matter" and "resistance" have for Whewell seems to originate in the fact that our "knowledge" of these terms is sometimes said to be derived from two sources--sensations and thought. The ambiguity, and Whewell's attempted resolution of it, are suggested in the following passage: "We have thus seen . . . the origin of our conceptions of Force, Matter, Solidity, and Inertness. It has appeared that the organ by which we obtain such conceptions is that very muscular frame, which is the main instrument of our perceptions of space; but that, besides bodily sensations, these ideal conceptions like all the others which we have hitherto considered, involve also an habitual activity of the mind, giving to our sensations a meaning which they could not otherwise possess." (Ibid., I, 191.) Elsewhere Whewell seems to drop the notion that our muscular frame is the organ by which we obtain a conception such as matter, "matter" being defined as "that which resists or reacts when acted on by force." (Ibid., II, 578.) When he is discussing the establishment of the principles of dynamics he claims that "the nature of matter is no more unknown to us than the nature of space or of numb r." (Ibid., I, 227.) The certainty which is here ascribed to our knowledge of matter is said to originate in the fact that our knowledge of matter is a knowledge of "certain relations which are the necessary groundwork of our knowledge." This view is in keeping with the view expressed in Whewell's dis-

cussion of the classification of the sciences where Force, Matter, and Inertia are listed as "Fundamental Ideas or Conceptions." (Ibid., II, 117.) Now, Ideas and Conceptions are not, Whewell maintains, derived from experience, but from thinking about it. Numerous passages could be cited to substantiate this contention. But perhaps one will suffice: "By the course of speculation contained in the last three Chapters, we are again led to the conclusion which we have already stated, that our knowledge contains an ideal element, and that this element is not derived from experience." (Ibid., I, 74.) The chapter from which the above sentence is quoted is entitled, "The Fundamental Ideas Are Not Derived From Experience."

26 For characteristic uses of the term "media," in the sense in which Whewell subscribes to media, see Philosophy of the Inductive Sciences, I, 277, and II, 228.

27 Immanuel Kant, Critique of Pure Reason, tr. N.K. Smith (London, 1950), A 370-A 371.

28 "The term intuition (in its most rigorous sense) is applicable only to that mode of contemplation in which we . . . look at objects as made up of parts, and apprehend the relations of those parts at the same time and by the same act by which we apprehend the objects themselves." (Whewell, Philosophy of the Inductive Sciences, I, 90.) Whewell does not, however, consistently employ the term in the manner here stipulated. He says, for example, that "intuition" might be used to refer to "those cases in which we necessarily apprehend relations of things truly as soon as we conceive the objects distinctly." (Ibid., II, 604.) He then goes on to say that in this sense axioms may be said to be known by intuition. And, as noted in the text, he also has an expression--"the sight which produces knowledge"--which signifies what today would generally be called "intuition.

29 William Whewell, Of Induction with Especial Reference to Mr. J. Stuart Mill's System of Logic (London, 1849), 37.

30 These axioms are said to be as follows. (1) Nothing can take place without a cause. (2) Effects are proportional to their causes, and causes are measured by their effects. (3) Reaction is equal and opposite to action. See Whewell, Philosophy of the Inductive Sciences, I, 178-185.

31 Mill, Logic, I, 15.

32 A theory, in the sense of a theory about the facts, must not be confused with what I called earlier a "theory of

knowledge." That knowledge consists in the relating of particulars is an example of what I mean by a theory of knowledge statement. The theory of gravitation is an example of what I mean by a theory about the facts. For documentation of the statement that theories may become facts, see, e.g., Whewell, Philosophy of the Inductive Sciences, I, 14, and On the Philosophy of Discovery, 444.

33 Whewell, Philosophy of the Inductive Sciences, I, 467.

34 Whewell, Of Induction, 34.

35 Whewell, Philosophy of Discovery, 345. Cf. Of Induction, 30-31: "[The Philosophy of the Inductive Sciences] is, in reality, no less historical than the History which preceded it. The History of the Inductive Sciences is the history of the discoveries, mainly so far as concerns the facts which were brought together to form sciences. The Philosophy is, in the first ten Books, the history of the Ideas and Conceptions, by means of which the facts were connected, so as to give rise to scientific truths."

36 Whewell, Of Induction, 76.

37 In support of his claim that the perception of distance involves an inference, Whewell cites Berkeley's, An Essay towards a new Theory of Vision. See Whewell, Philosophy of the Inductive Sciences, I, 112, 116, 287.

38 Whewell, Philosophy of Discovery, 366.

39 Ibid., 300.

40 To employ the term "object" to refer to both ideal and sensible entities may seem ambiguous, but it appears to be consistent with Whewell's usage. This terminological problem is discussed by him in an essay entitled, "Of the Intellectual Powers" reprinted in the Philosophy of Discovery. Note the phrase "mental objects" in the following passage from Philosophy of Discovery, 444: "In using the term Ideas to describe the mental sources from which Conceptions derive their validity in demonstration, I am employing a phraseology which I have already introduced in the Philosophy of the Inductive Sciences. But independently altogether of this, I do not see what other term could be employed to denote the mental objects, attributes, or powers, whatever they be, from which Conceptions derive their evidence, as Demonstrative Truths derive their evidence from Intuitive Truths."

41 Whewell, Philosophy of the Inductive Sciences, I, 291.

42 *Ibid.*, 611-612.

43 Whewell, *Philosophy of the Inductive Sciences*, I, [illegible].

44 Whewell, *Of Induction*, 75-76.

45 J.K. Feibleman, "Datum." D.D. Runes, *op. cit.*, 77.

46 Whewell, *Philosophy of the Inductive Sciences*, II, [illegible].

47 See William Whewell, *Novum Organon Renovatum* (London, 1858), 114. *Cf.*, Whewell, *Philosophy of Discovery*, 482.

48 Whewell, *Novum Organon Renovatum*, 11[illegible].

49 For example, from a knowledge of the ellipse in which Mars travels, one can deduce the position of Mars in the heavens at any time in the future.

50 Whewell, *Philosophy of Discovery*, 350.

51 *Ibid.*, 371.

52 Whewell discusses this problem in many places. In the present chapter I shall follow, for the most part, the discussion in the *Philosophy of the Inductive Sciences*, Bk. XIII: "Of Methods Employed in the Formation of Science."

53 Whewell, *Of Induction*, 14.

54 Whewell, *Novum Organon Renovatum*, 115.

55 Whewell, *Philosophy of the Inductive Sciences*, II, [illegible].

56 Whewell, *Of Induction*, 15.

57 Whewell, *Philosophy of the Inductive Sciences*, II, 27.

58 *Ibid.*, 30.

59 *Ibid.*, 31.

60 *Ibid.*

61 *Ibid.*, 33-34.

62 *Ibid.*, 738.

63 *Ibid.*, 446.

64 *Ibid.*, 382.

65 *Ibid.*, 6-7.

66 *Ibid.*, 16.

67 *Ibid.*, 613.

68 *Ibid.*, 380.

69 The examples in this paragraph are Whewell's. See *Philosophy of the Inductive Sciences*, II, 382.

70 Whewell, *Philosophy of the Inductive Sciences*, II, 38[illegible].

71 *Ibid.*, 383.

72 Whewell, *Philosophy of Discovery*, 29.

73 Whewell, *Of Induction*, 37.

74 See Whewell, *Philosophy of the Inductive Sciences*, II, 117.

75 See *ibid.*, I, 166-185.

76 See *ibid.*, II, 421-440.

77 *Ibid.*

78 *Ibid.*, 425.

79 *Ibid.*, I, 166.

80 See *ibid.*, 167-170.

81 *Ibid.*, 169.

82 *Ibid.*, II, 445.

83 *Ibid.*, I, 175.

[64] Whewell, <u>Philosophy of Discovery</u>, 369-370. I have a few criticisms of Whewell's usage of the expressions "First Cause" and "final Cause." I shall now deal with these terms in order.

Leaving aside the usual criticisms which can be raised against the various cosmological proofs for the existence of a First Cause, we can still be critical of Whewell's usage of the term "First Cause" because this usage introduces certain ambiguities into the term "cause". In the first place, while we may be able to conceive the First Cause as a power or force it is not clear what sort of demand is made here by Whewell upon our thought. The particular force which he calls the First Cause is given the negative characteristic that it is not included within the course of nature. Although it is the beginning of the present cycle of organic nature it is not itself part of that cycle. But how we are to think of it, or conceive it, <u>qua</u> force I do not know. It is clear that if it be non-natural we cannot conceive it by means of analogy from the natural causes with which we are familiar. Secondly, natural causes have the characteristic that they are necessary, that is to say, only such and such causes produce such and such effects. But this particular kind of necessity Whewell does not succeed in demonstrating in the case of the First Cause. Thirdly, Whewell tells us that all the palaetiological sciences point to the same First Cause. Now, it seems to me that this introduces a very difficult connotation into the idea of cause. The idea of a cause capable of producing any effect whatever is not foreign to the idea of cause "in general." I, for example, have no difficulty in thinking of energy in this way. But I am not sure how scientific such a cause is. In science, at least as Whewell expounds it, "cause" usually means the cause of some specific effect. The specific effect of which the First Cause is the cause is the universe taken as a whole. It is very difficult to take as a datum the universe as a whole. But, leaving this difficulty aside, not only is the First Cause said to be the cause of the universe as a whole, it is also said to be the cause of all that occurs within the universe. And this makes for confusion. For example, I persuade myself that gravitation is a cause of the motions of the planets. And, at the same time, I hold that the First Cause is <u>the</u> cause of motion of the planets. If gravitation were said to be a "form," or species, of the First Cause, in the way in which light is a "form" of energy, it would then be easier to conceive gravitation and the First Cause as causes of the planetary motions. But, in the form in which Whewell has expressed his views about the First Cause, I do not find his views intelligible.

My opinion is that Whewell is here attempting to include within one order of knowledge, statements which do not belong to the same order. If we take Whewell's <u>Mechanical Euclid</u> as an instance of first order knowledge, then his statements about gravitation belong to second order knowledge, because they pro-

vide the ground of his first order knowledge. He also tries to include statements about the First Cause as second order knowledge, when, actually, such statements belong to a third order of knowledge. Or, at any rate, it is my opinion that such statements belong to third order knowledge. For such statements provide the ground for the theory of gravity. Whewell does not accept the force of gravity as an ultimate explanatory cause or force. Instead he asks, What is the cause of it? And his answer is that the cause of gravity is the First Cause. To refer again to the analogy I have employed above, I might accept light as the cause of the process called photosynthesis. But I also might ask, What is the cause of light? And to this last question I might give the answer, Energy. My knowledge of energy as cause, and my knowledge of light as cause, therefore, when expressed in this way, belong to two different orders, with the former being the ground of the latter. Similarly I feel that Whewell's knowledge of a First Cause is not of the same order as his knowledge of the Newtonian laws of Motion. Whewell does not make this distinction. But I think his expositi n would be clearer, and more defensible, if he had done so.

Whewell's usage of the Idea, First Cause, seems to me roughly synonymous with his usage of the Conception of Force. As a Conception it can, perhaps, be made precise and be said to be this particular Conception, whereas as an Idea it is very difficult to give it this precision.

A further objection to Whewell's usage of First Cause is that it is difficult to correlate this usage with the general notion of cause as a colligation of facts. I think the difficulty arises here primarily because the First Cause is said to be not included in the course of nature. I fail to see, therefore, how we can possibly have any inductive verification of assertions concerning the First Cause.

In the matter of final causes, although I cannot accept some of Whewell's more extreme statements as, for example, that in nature nothing happens in vain and that obviously the eye was designed, by a Divine Intelligence presumably, for seeing and the ear for hearing, nevertheless I am sympathetic to the idea that in order to account for some behaviour on the organic and psychological levels it is necessary to invoke final causes. However, my chief interest at the moment is not to argue for or against final causes nor to appraise Whewell's arguments in support of them, but, rather, to discuss them within the context of Whewell's usage of the term "cause." There are three points which I should like to make here. One is that it is difficult to conceive final causes as forces. I am reasonably certain that Aristotle, whose arguments Whewell is obviously paraphrasing, does not think of final causes as forces, but rather as factors which have to be taken into consideration. They are ends in nature and, as such are differentiated from efficient causes. And my complaint against Whewell here is that he fails to say whether or not final causes are to be regarded as forces, or as some-

thing else. Secondly, from Whewell's account of final causes, I should say that to use the term "cause" is a misnomer. What he calls final causes are, at best, laws of organic behaviour, not causes of the behaviour. Thirdly, there is also in Whewell the notion of a Final Cause. In this sense the final Cause stands for the will and purpose of a divine and supernatural Creator. And against a final Cause in this sense the objections raised above in relation to a First Cause seem pertinent.

85 Mill, to a certain extent, also accepts the possibility of there being two kinds of definition, definitions of names, which explain the meaning of terms, and definitions of things, which explain the nature of things. But Mill concludes that definitions are properly only of names, not of things. See _Logic_ I, 160-170.

86 See, for example, _Philosophy of the Inductive Sciences_, II, 424: "When we have established Natural classes of objects, we seek for characters of our classes, and these Characters may, to a certain extent, be called the _Definitions_ of our classes. This is to be understood, however, only in a limited sense: for these Definitions are not absolute and permanent. They are liable to be modified and superseded. If we find a case which manifestly belongs to our Natural class, though violating our Definitions, we do not shut out the case, but alter our definition."

87 _Ibid._, I, 480.

88 _Ibid._, II, 372.

89 _Ibid._, I, 481.

90 _Ibid._, I, 528.

91 _Ibid._, 477. Italicized in the original.

92 _Ibid._, 477.

93 Here Whewell takes the same attitude to general names that Mill does. The name does not name a "universal," but a collection of individuals.

94 Whewell, _Philosophy of the Inductive Sciences_, I, 475. My italics.

95 _Ibid._, 476.

96 Ibid., 476.

97 Ibid., 576.

98 For a discussion of the employment of these principles in this way see ibid., 46?-473.

99 Ibid., 72-77.

100 That it is not unthinkable is shown by the fact that some persons have been able to think it. See e.g., ibid., 16?-166.

101 Whewell, Philosophy of Discovery, 339.

102 Ibid., 340-350.

103 Whewell, Philosophy of the Inductive Sciences, II, 4.

104 Ibid., 360-387.

105 Whewell, On Induction, 21-22.

106 Whewell, Philosophy of Discovery, 701.

107 See William Kneale, "Induction, Explanation, and Transcendent Hypotheses," in Herbert Feigl and May Brodbeck (eds), Readings in the Philosophy of Science (New York, 1953), 357-358.

108 See Whewell, On Induction, 61-62. Cf. Philosophy of the Inductive Sciences, Bk. IX, Ch. V, Art. XI.

109 See 62-63, and 108, above.

110 See 49, 58, 62-63, and 169-170, above.

111 Whewell, Philosophy of the Inductive Sciences, I, 77.

112 See ibid., II, 669-670.

113 Ibid., II, 670.

114 See Whewell, "Remarks on a Review of the Philosophy of the Inductive Sciences," Philosophy of the Inductive Sciences, II, 669-670.

115 Whewell, <u>Philosophy of the Inductive Sciences</u>, 677-686.

116 <u>Philosophy of Discovery</u>, 364, provides an example: "We apply then to Force and Matter the doctrine--the Platonic doctrine, if any one please so to call it,--that the world is constituted according to the Ideas of the Divine Mind, and that the human mind apprehends the inward and most fundamental relations of the universe by sharing in some measure of those same ideas.

"But do we go on with Plato to extend this doctrine of Ideas to all the objects and all the aspects which constitute the material universe? Do we say with Plato that there is not only an Idea of a Triangle by conformity to which a figure is a triangle, but an Idea of Gold, by conformity to which a thing is gold, and Idea of a Table, by conformity to which a thing is a table? We say none of these things."

117 See 56-58, above.

118 Whewell, <u>Philosophy of the Inductive Sciences</u>, I, 68-69.

119 <u>Ibid</u>., 69.

120 <u>Ibid</u>., II, 595.

121 <u>Ibid</u>., 596.

122 <u>Ibid</u>., 606.

123 <u>Ibid</u>., 604.

124 <u>Ibid</u>., 604.

125 The distinction between "purely formal sciences" and "merely formal sciences" is essentially mine, not Whewell's, even though he does use the two expressions. Nevertheless, the distinction merely clarifies the point which Whewell makes. It does not add to, or distort, his account. The point is that a science must always be about either the objects of perception or the forms under which objects are perceived.

126 Whewell, <u>Philosophy of the Inductive Sciences</u>, II, 451.

127 <u>Ibid</u>., I, 150; "Space and Time . . . are the subjects of pure mathematics."

128 Ibid., II, 596.

129 Ibid., I, 150.

130 My interpretation is that "ideas" should be spelled with a capital "I" throughout this passage.

131 Whewell, Philosophy of the Inductive Sciences, II, 653.

132 Whewell, Philosophy of the Inductive Sciences, I, 121-122. It is interesting to note that John Stuart Mill appears to accept Reid's claim that if we had only the sense of sight, the "Geometry of Visibles," as expounded by Reid, would follow. See John Stuart Mill, An Examination of Sir William Hamilton's Philosophy . . . (Boston, 1866), I, 30, n.

133 William Whewell, History of the Inductive Sciences from the Earliest to the Present Times (London, 1857), I, 16.

134 A representative passage on this point is the following: "The pure Mathematical Sciences must be successfully cultivated, in order that the progress of the principal Inductive Sciences may take place. This appears in the case of astronomy, in which Science, both in ancient and in modern times, each advance of the theory has depended upon the previous solution of problems in pure mathematics. It appears also inversely in the Science of the Tides, in which, at present, we cannot advance in the theory, because we cannot solve the requisite problems in the Integral Calculus." Whewell, Philosophy of the Inductive Sciences, II, 481.

135 Actually, it is deduction of a particular sort--the Concrete Deductive Method--that Mill argues for. This method is discussed later in the thesis. To the extent that it includes hypotheses it resembles Whewell's concept of induction.

136 Mill, Logic, I, 59. It is interesting to note that perceptions are not included in the above list. On this point Mill says, "What are called perceptions are merely a particular case of Belief, and belief is a kind of thought." Ibid., 82.

137 See ibid., 81-83.

138 See ibid., 82.

139 Ibid., 6.

140 See 218-226, below.

141 See, for example, his reference to, "Metaphysics, that fertile field of delusion propagated by language." Mill, Logic, I, 14.

142 The difference in terminology here is this. A permanent cause is a temporal antecedent postulated only as permanent. An original natural agent is a temporal antecedent posited not only as permanent but also as a force or power--the force, or power, necessary to account for the appearance of the effect.

143 Mill, Examination of Hamilton, I, 60. Mill's book on Hamilton is, in large measure polemical. See, e.g., Michael St. John Packe, The Life of John Stuart Mill (London, 1954), 442: "He [Mill] proposed to write an article for the Westminster on the philosophy of Sir William Hamilton, who seemed to him to be making a praiseworthy attempt to reconcile the rival systems of the Transcendentalists and the Scottish school of Common Sense. But a preliminary survey of the works of that great pedant revealed, behind the bulwark of his erudition a sea of error so expansive and so menacing, that Mill determined grimly on a full examination." However, I am not interested in the polemical quality of the book on Hamilton. I am interested in the book only as a sequel to the Logic. According to Packe, Mill regarded the Examination of Hamilton as a metaphysical sequel to the Logic. See Packe, ibid.

144 Mill, Logic, II, 705. My italics.

145 "Comte . . . was content to describe the methods of science; Mill wished to justify them." John Passmore, A Hundred Years of Philosophy (London, 1957), 15.

146 See Mill, Logic, I, 5.

147 Ibid., 718.

148 Ibid., 99.

149 Ibid., 103.

150 Ibid., 106.

151 Ibid., 115.

152 See O.A. Kubitz, "The Development of John Stuart Mill's System of Logic," in E.L. Bogart, J.A. Fairlie, and A.H. Lyler, (eds.), Illinois Studies in the Social Sciences XVII (Urbana, 1932), 65.

153 Ibid., 86.

154 Kubitz has a good account of this. See Kubitz, op. cit., 87-89.

155 Mill, Logic, I, 85.

156 Ibid.

157 Ibid., 85-86.

158 Ibid., 86.

159 Ibid.

160 Ibid.

161 Ibid.

162 Ibid., 116. My italics.

163 Mill, Examination of Hamilton, II, 161.

164 L.S. Stebbing, A Modern Introduction to Logic (London, 1930), 157.

165 Gilbert Ryle, "The Theory of Meaning," in Mace, op. cit., 242.

166 Ibid., 246.

167 Ibid., 243-247. I do not find Ryle entirely clear on this point. But I understand him to be saying that Mill almost succeeded in avoiding the fallacy referred to, but did not quite succeed in avoiding it.

168 White, op. cit., 61.

169 W. Windelband, A History of Philosophy (New York, 1914), 635.

170 I am here employing Ryle's way of stating the different senses of "exist." See Ryle, The Concept of Mind (London, 1949), 27. Quoted in White, _op. cit._, 62.

171 Mill, quoted in Stebbing, _op. cit._, 157-158.

172 See Stebbing, _op. cit._, 152.

173 _Ibid._

174 Mill, _Logic_, I, 124.

175 _Ibid._, 56.

176 Mill, _Examination of Hamilton_, I, 242. _Cf._, _ibid._, 240.

177 _Ibid._, 236.

178 See Mill, _Logic_, I, 67.

179 _Ibid._, 68.

180 _Ibid._, 219.

181 "All things which possess extension, or in other words, fill space, are subject to geometrical laws. Possessing extension, they possess figure; possessing figure, they must possess some figure in particular, and have all the properties which geometry assigns to that figure." _Ibid._, 360.

182 _Ibid._, 98.

183 See John Stuart Mill, _Auguste Comte and Positivism_ (London, 1866), 62: "He [Comte] rejects totally, as an invalid process, psychological observation properly so called, or in other words, internal consciousness . . . . Our knowledge of the human mind must, he thinks, be acquired by observing other people . . . . It is clear to him that we can learn very little about the feelings, and nothing at all about the intellect, by self-observation. Our intelligence can observe all other things, but not itself: we cannot observe ourselves observing, or observe ourselves reasoning: and if we could, attention to this reflex operation would annihilate its object by stopping the process observed."

184 John Stuart Mill, _Autobiography_ (New York, 1873), 169.

185 In addition to Book VI of the Logic, see Autobiography 210-211, where Mill acknowledges his debt to Comte in this area, and also defines the inverse deductive method.

186 Philipp Frank, Philosophy of Science (Englewood Cliffs, N.J., 1957), 297.

187 Mill, Logic, I, iii-iv. A slightly different statement of the nature of philosophy is to be found in Auguste Comte and Positivism, 53: "The proper meaning of philosophy we take to be, what, in the main, the ancients understood by it--the scientific knowledge of man, as an intellectual, moral, and social being. Since his intellectual faculties include his knowing faculty, the science of Man includes everything that man can know, so far as regards his mode of knowing it: in other words, the whole doctrine of the conditions of human knowledge. The philosophy of a Science thus comes to mean the science itself, considered not as to its results, the truths which it ascertains, but as to the processes by which the mind attains them, the marks by which it recognizes them, and the co-ordinating and methodizing of them with a view to the greatest clearness of conception and the fullest and readiest availability for use; in one word, the logic of the science." And on the following pages (54-55) we find another of Mill's infrequent usages of the expression "philosophy of science": "The philosophy of Science consists of two principal parts; the method of investigation, and the requisites of proof. The one points out the roads by which the human intellect arrives at conclusions, the other the mode of testing their evidence. The former if complete would be an Organon of Discovery, the latter of Proof. It is to the first of these that M. Comte principally confines himself . . . . We have been taught the right way of searching for results, but when a result has been reached, how shall we know that it is true? How assure ourselves that the process has been performed correctly, and that our premises, whether consisting of generalities or of particular facts, really prove the conclusion we have grounded on them? On this question M. Comte throws no light. He supplies no test of proof."

188 Mill, Logic, II, 524.

189 Grote states this objection very concisely: "Mr. Mill, I think, is rather too free in his references to a supposed 'metaphysics', to which the consideration of certain fundamental difficulties belongs. That is to say, he does to a certain extent deal with such difficulties, to such an extent, it seems to me, as to preclude himself from saying with reason that they belong to a different subject from that which he treats of." John Grote, Exploratio Philosophica (Cambridge, 1900), 209, n.

190 Mill, Logic, I, 67.

191 Grote calls this sort of thing Mill's "notional ontology": "And what I differ from Mr. Mill in . . . is this, that while condemning apparently Ontology, or the notion that bodies have a super-perceptional constitution which we may hope to find out, he seems to countenance the belief that they have one, which, from its very nature it is useless for us even to try to find out. This is the 'notional' Ontology, if I may so call it, which seems to me worse than an attemptedly Real Ontology." Grote, *op. cit.*, 185-186.

192 Mill, *Logic*, I, 112.

193 *Ibid.*, II, 422.

194 *Ibid.*, 4.

195 See, for example, Mill, *Logic*, I, 472: "I myself, in common with Dr. Whewell, have maintained against the purely empirical school . . . ." And so on.

196 See Mill, *Logic*, I, 6.

197 Mill, *Examination of Hamilton*, I, 159.

198 *Ibid.*, II, 221.

199 Mill, *Logic*, 8th ed. (London, 1949), 271. My italics.

200 For a fuller discussion, and for examples, see especially *Logic* I, 314-318, and *Logic* II, 8-27, 481-500.

201 Mill, *Logic*, I, 319.

202 *Ibid.*, 347.

203 Mill certainly recognized that there was a problem here. But he seems to have felt that little could be done to resolve it. "Invention, though it can be cultivated, cannot be reduced to rule; there is no science which will enable a man to bethink himself of that which will suit his purpose. But when he *has* thought of something, science can tell him whether that which he has thought of will suit his purpose or not." Mill, *Logic*, I, 315. Since checking the validity of an argument can be reduced to rules, it is to this problem that Mill devotes his attention.

204 Mill, *Logic*, 8th ed., 283.

[205] Such is the opinion of Anschutz. And I agree that Anschutz is correct here. See R.P. Anschutz, _The Philosophy of J.S. Mill_ (Oxford, 1953), 97-99.

[206] "Explanation" is a technical term with Mill. To explain means to substitute laws of greater universality for laws which are less universal. There are, of course, "ultimate" laws, _i.e._ laws which can be used as principles of explanation, but which, themselves, cannot be explained. But these "ultimate" laws are not empirical laws. Ultimate laws, are, instead, laws relating to "original natural agents." Gravity is an "original natural agent" and the laws of gravity are, therefore, "ultimate" laws. Since the laws of gravity are ultimate, they cannot be explained. But they can be verified. Their verification consists in deducing less general laws from them, and, finally empirical laws from these. But, ultimately, it is the empirical laws which constitute proof. We can verify the empirical laws intuitively, _i.e._, by a direct inspection of experience. And it is here that we finally have to take our stand. If from a law of nature we can deduce law which we know, on the basis of experience, to hold universally, this constitutes verification of the law of nature.

[207] Mill, _Logic_, II, 101.

[208] For a forceful criticism, and rejection, of Mill's arguments to the effect that the Law of Universal Causation or the Uniformity of Nature can be established inductively, see H.W.B. Joseph, _An Introduction to Logic_ (Oxford, 1906), 386-391.

[209] Mill, _Logic_, I, 11.

[210] When we define the cause of anything (in the only sense in which the present inquiry has any concern with causes) to be 'the antecedent which it invariably follows' we do not use this phrase as exactly synonymous with 'the antecedent which it invariably has followed in our past experience.' . . . But it is necessary to our using the word cause, that we should believe not only that the antecedent always _has_ been followed by the consequent, but that, as long as the present constitution of things endures, it always will be so." Mill, _Logic_, I, 375-376.

[211] See Mill, _Logic_, II, 11-12.

[212] Mill, _Examination of Hamilton_, II, 110. Packe has a comment on this point which is worth noting. The law of kinds

is an empirical law and is, therefore, valid only within the conditions of time, space, and circumstance in which it is experienced. The law of natural kinds, therefore, belongs to the same class of laws as the laws of the abstract sciences--the so-called necessary truths--and the law of universal causation. See Packe, op. cit., 258.

213 Mill, Examination of Hamilton, II, 76.

214 Ibid., 95. My italics.

215 Mill, Logic, II, 261.

216 Ibid., I, 205.

217 Ibid., 204.

218 Ibid., 251-252.

219 Ibid., II, 11.

220 Ibid., 483.

221 "Some theorists, mistaking it--/(sociology)/-- for an experimental subject such as chemistry, had approached it by induction. They had taken examples from the historical past, and from them built experimental laws which they then verified, or attempted to verify from human evidence in the present day. They had forgotten that the experimental method required laboratory precision not found in the confused morass of history, where all the facts could never be completely tabulated, and where it was impossible to find two cases that were exactly parallel. As a result, they had achieved a philosophy of history totally divorced from the characteristics of single human beings. Inevitably, by generalizing on the species without sufficient study of its members, they were led to set man in society above man as an individual creature. The organic theory of the state appeared to Mill a fallacy of hideous propensities. Society was a mixture, not a compound; a bag of bullets, not a molten ingot . . . . Another group of sociologists who were in error were those who, while rightly perceiving that the science ought to be deductive, had made the mistake of taking geometry for their model. From very few, or even from a single principle of human nature, they set up rigid and purely abstract laws to apply at all times and in all places. In this class came the Benthamites, and especially his own father: they, basing all upon self-interest, had sought to make a permanent system out of a creed admirably suited to achieve the particular purpose of the Reform Bill. In truth,

is not a thought, but a fact concerning the things in the world." Bertrand Russell, The Problems of Philosophy (London, 1956), 88-89.

225 Mill, Logic, I, 201.

226 Mill, Logic, I, 287.

227 See Mill, Autobiography, 215-216.

228 Albert Einstein, "Geometry and Experience," in Feigl and Brodbeck, op. cit., 189.

229 See Mill, Logic I, 267-284, and Logic, II, 137.

230 Mill, Examination of Hamilton, II, 141.

231 It is, of course, prescriptive as rule of logic. However, it does not originate as an arbitrary prescription but, rather, as an induction.

232 Mill, Examination of Hamilton, II, 157.

233 Mill, Logic, I, 7.

234 On this point see Packe, op. cit., 441: "Memory was the present consciousness of a past sensation. But since he could not further explain it, he was forced very grudgingly to admit it as another unaccountable faculty of mind . . . . Much to the disgust of Bain. 'For myself,' said Bain, 'I never could see where his difficulty lay.'"

235 This seems to me the usual view which Mill adopts in the Logic. Logic, I, 60-76 provides documentation for this view.

236 No doubt this statement will prove much too idealistic for many of Mill's disciples. However--and, of course this certainty does not prove that Mill is an idealist--I, for one, am not surprised to learn that "Berkeley was Mill's favourite among the old philosophers." See Packe, op. cit., 478. See also: "Matter, then, may be defined, a Permanent Possibility of Sensation. If I am asked whether I believe in matter, I ask whether the questioner accepts this definition of it. If he does, I believe in matter: and so do all Berkeleians. In any other sense than this I do not." Mill, Examination of Hamilton, I, 243. My italics.

It is my view that the claims set forth in the paragraph to which this footnote is appended represent the meta-meta-theory of the Examination of Hamilton. Mill, however, appears to assert that all that he requires as ontologically real, in the position which he adopts in this book, are feelings and the permanent possibilities of feelings. It seems to me that it would be quite easy, on this view, to show that the permanent possibilities are also only feelings: the permanent possibilities are inferences; inferences are thoughts; thoughts are feelings; therefore, the permanent possibilities are feelings. But if Mill were to permit this "reduction" then he would be hard put, I think, to translate into feelings what is signified by such expressions as "I think," "I know," "I infer," "I conclude," "I remember," and so on. I am of the opinion, therefore, that the position which Mill takes up in the Examination of Hamilton requires, as I have said, subjects, the dispositional powers of subjects, and the content of the consciousness of subjects. As documentation for this view I shall cite especially pages 245-256 of the Examination of Hamilton.

237 It is of course necessary to remember, as has been pointed out earlier, that although for Whewell the mind knows certainly the forms of the knowledge it possesses, it does not thereby come into possession of a knowledge which is complete.

238 See Mill, Examination of Hamilton, II, 186-188. Note especially ibid., 180.

239 Sir William Hamilton, The Works of Thomas Reid, D.D. . . . . (Edinburgh, 1880), II, 762.

240 Mill, Examination of Hamilton, II, 160-161.

241 John Dewey says that this is Mill's view. And I agree that it is. See John Dewey, "An Empirical Survey of Empiricisms," in Studies in the History of Ideas, edited by The Department of Philosophy of Columbia University (New York, 1935), III, 20.

242 Mill does not supply such a classification. Whewell does. I have discussed classifications which would be consistent with Mill's views. But I have said nothing about Whewell's classification. Classification of the sciences can, according to Whewell, proceed upon a number of principles. The sciences can be classified according to method. Such a classification yields the pure sciences and the empirical sciences. They can be classified according to subject matter, and especially according to the Fundamental Ideas upon which the various sciences are grounded. Such a classification of

the sciences yields the pure mathematical sciences, the pure motional sciences, the mechanical sciences, the secondary mechanical sciences, the analytico-mechanical sciences, analytical science, the analytico-classificatory sciences, the classificatory sciences, the organical sciences, and the palaetiological sciences. This is the classification which he finally adopts. (See Whewell, Philosophy of the Inductive Sciences, II, 117.) Or they can be classified in terms of the degree of generality which they exhibit. However, Whewell does not succeed in doing much with this notion. All he succeeds in doing is to set up two sciences--astronomy and optics--in such a way as to show the progress of those sciences from propositions asserting observations of objects to propositions stating very general laws.

Whewell is seriously hindered in his attempt to classify the sciences because of his belief in the independence of the Ideas which form the bases of sciences, and by his belief that each science has its own proper conceptions and axioms. For example, he rejects the possibility of explaining biological phenomena in chemical or physical terms. The relations among the sciences, therefore, remain unclear.

Whewell admits the possibility of social sciences and thinks that they will have to be set up on the model of the sciences already achieved. But he does not see the social sciences as extensions of any of the existing sciences. They will have their own relevant data and their own appropriate Ideas from which will flow the conceptions, definitions, and axioms pertinent to them.

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